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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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EDITORIAL

PARAPSYCHOLOGY AND SCIENTIFIC RECOGNITION

As of 1952, parapsychology has definitely failed to impress the majority of American psychologists. There have been various indications of this general lack of recognition from time to time, but now, if we take Dr. Warner's survey (published in this issue) as representative, we cannot possibly escape the fact. Even when the question of the occurrence of ESP is broadened, as it was in the Warner questionnaire, to the extent of asking whether ESP is "a likely possibility," only one psychologist in six on the average is prepared to agree. What is especially sobering is the fact that according to Warner's comparison with his survey of fourteen years ago, this percentage of 16.6 is little more than 8 percent above what it was for the same question in 1938. When we consider along with this the fact that the greatest bulk of the best controlled work in ESP has been done during the fourteen years between surveys, we are forced to conclude that something must be wrong, quite apart from the evidence itself.

Indeed, it can almost be concluded, on the strength of Warner's figures, that this matter of acceptance is not a question of the evidence. This conclusion would have such grave implications regarding a large body of professional scientific men that it ought not to be made without due caution. But we do know of certain individuals, some of them capable scientists in other respects, who have been frank to say that no amount of evidence could convince them of the occurrence of ESP. One of these, a psychologist and friend who

has actually witnessed a successful ESP performance under conditions he himself laid down for the subject, has told us of his reply to a question put to him by another scientist as to what he thought of the case for ESP. "If it were on any other issue, one-tenth of the evidence reported would have been enough to convince me. As it is, ten times that amount would not do it."

From the Warner report we now see that this was not just an isolated case. According to the survey, although five out of six psychologists do not recognize ESP as a likely possibility, *only a little over an average of one in six claim to have based their opinions on any acquaintance with the scientific reports!* Seventeen percent in both surveys even admitted to having reached their opinions on the basis of hearsay. But the really significant revelation, to our judgment, is the fact that approximately *20 percent in both surveys put on the record that they reached their conclusions entirely on a priori grounds;* they had their minds already made up on the question in advance. They admitted they were not judging the case by the evidence. (Let us pause to remind ourselves that these are, for the most part, university scientists, the professionally trained men and women who are responsible for research and instruction here in America in the field of psychology, the science that should be, and doubtless will come to be, the greatest of all in its importance to mankind.)

But even as we in parapsychology concede this failure to gain acceptance of our findings by the profession most concerned, we must face the situation squarely and learn what we can from it. A well-interpreted failure can be of positive value. Let us then examine the possible interpretations. To take first the easiest way out, we could put the entire responsibility on the psychologists themselves and indict all those who will not look at the evidence of psi as simply not being good scientists. We might say they are not really open-minded to new experimental facts, especially if those facts are disturbing; that they prefer a complacent peace of mind to any upsetting knowledge of mind; and that they are unable to decide professional issues with proper scientific detachment. But while there might be a great deal of truth in such an indictment, an objective view of the situa-

tion would not, we believe, justify it as a balanced and complete analysis.

We could, on the other hand, put the principal blame on the workers in our own field. Since the findings in parapsychology are bound to be revolutionary if accepted, the psi investigator is naturally inclined to picture their significance in strong terms. The very intensity of the explorer's interest may easily lead him to appear to exaggerate the importance of his discoveries. Such an attitude may put the average psychologist on the defensive and tend to make recognition impossible. This second interpretation, like the first, could be rated as not entirely wrong but, again, we are confident it does not represent a fair and adequate picture of the situation. So let us try a third.

The beginning of the explanation lies, we believe, in the obvious fact that psychologists constitute a group of highly specialized professional men and women who are already intensely preoccupied with an area of absorbing problems; their work allows them little time to go probing into all the frontier problems of the enormously expanded field represented by psychology today. (For that matter, very few if any of the workers in parapsychology are keeping up with all the advances in the other psychological specialties.) Moreover, these Fellows of the American Psychological Association who responded to Professor Warner's questionnaire are mostly mature, settled men who may no longer be keenly looking for a new approach to the question of the nature of man; they are, perhaps, somewhat resigned, if not attached, to their own working philosophies of human nature.

Now, we are all more or less guided by some sort of general working philosophy. It is true of scientists in every field. Psychologists are dominated, in a major way, by a working philosophy derived from the physical sciences. They are especially sensitive to anything that threatens to tie up their newly developing science to its older relationships with the occult, mystical, or supernatural. In the face of claims that even so much as suggest a throwback to these bygone associations with superstition, the psychologist puts up his maximum resistance and clings tenaciously to the physicalistic formulations that make up his guiding philosophy.

There is probably no clearer revelation of this working philosophy and its effect on American psychology than that given in the paragraph quoted from Dr. D. O. Hebb on the back cover of our March issue this year. In it Dr. Hebb, too, rejected the case for ESP but conceded that on almost any other issue the evidence would be convincing. "We are," he went on to say, "still trying to find our way out of the magic woods of animism where psychology began historically, and we cannot give up the talisman of a knowledge of material processes." In other words, before ESP can be accepted it will be necessary to find a materialistic explanation of it.

Dr. Hebb is right, we think, in his stand that psychology cannot accept ESP and still hold to a physicalistic philosophy of man. If he is representative of his profession, our quarrel, then, is with the psychologist's philosophy; and psychology's difficulty over ESP is concerned not with the evidence—neither its amount nor its quality—but with its implication for his philosophy. The question, therefore, is whether American psychology is to be first of all a philosophy or whether it is ready yet to be a science. A philosophical position, as Dr. Hebb frankly indicates, determines whether or not empirical findings are acceptable. In a full-fledged science it is supposedly the experimental findings that determine the generalizations to be made, even the very philosophy ultimately reached. These considerations are elementary to science.

Let us turn now to the parapsychologists and their contribution to this failure to win acceptance. If we can generalize about so small a group of necessarily highly individualistic explorers we can, in retrospect, recognize where parapsychology's trouble with psychology began and even how it may conceivably have been avoided. Perhaps the worst thing that was done was to allow the psi research to become associated with psychology's very worst bogey—dualism. When parapsychologists brought in findings that physics could not explain, they *could* as well have stressed the equally true and equally important *unitary* aspect of their nature and emphasized the *integration* of psi processes within the whole of personality and nature. The work *might* have been presented, we now see, as simply a further extension of man's known psychophysical adaptation to his environment, based, of course, on other still unknown principles that have

already been acknowledged. In short, psi workers need not have tried to upset the philosophical applecart of contemporary psychology. It was not necessary to go so far in pushing the implications of psi; it was not the way to get consideration and win approval.

At it was, the divergent character of the psi process was hailed as the significant one. It was mainly *because* of its defiance of physical explanation that psi interested most explorers in parapsychology. For that very reason, consequently, there was no desire to appease a materialistic psychology that had given us behaviorism and had too long dominated the schools of psychology, almost excluding the science from its own proper domain of subjective experience. Here was the answer to dogmatic physicalism, in psychology or out of it. Why should parapsychologists be restrained about such a happy discovery? They may, indeed, have been tactless, but the kind of people who successfully assail old conventional barriers and overcome resistances and other difficulties to do it must be expected to be vigorous and aggressive.

What is still worse, the few isolated workers in parapsychology, struggling against odds, have been compelled to try to win support for their work from some source. They have endeavored to justify their program by calling attention as they went to the meaning their findings have for certain fields of human relations—for example, religion—emphasizing, naturally, the importance of the experimental refutation of materialism. But to the mechanistic psychologist whose philosophy tells him in advance that ESP cannot occur, such an appeal is certain to appear groundless, if not fantastic. It only reinforces his resistance to the facts.

It is hard to say at this stage whether such appeals for co-operative interest were justified or whether, in the long view, the orientation of psi research should have been directed from the start solely toward winning over the professional psychologist. But from the history of psychology one gets the impression that in the past, at least, existing philosophies and schools have always had to be bypassed by new ideas. Old concepts have not been fluid and adaptive enough to be altered by new findings that challenged their foundations.

The short experience of parapsychology seems to bear out that

impression. Its critics have almost never been won over as their criticisms have been met and answered; they have merely been silenced and have been known even to return to their outworn and already answered utterances a decade later as to an "uncompleted task."

If this analysis has been essentially correct, it will at least partially explain the failure of parapsychology to secure recognition among psychologists. In brief: that failure is mainly a consequence of the strong materialistic philosophy that dominates psychological thinking in this country, combined with the antiphysicalistic challenge which the psi explorer in his provocative interpretation of the psi results has literally hurled at the psychology profession and at scientists in general.

What are the "sweet uses" of this adversity? What jewel may we hope to find in the toad's head which this failure represents? There is certainly no good to be gained from complaints and regrets, either about the dogmatic resistance of the psychologist or the aggressive radicalism of the parapsychologist. Nor is there any point in appealing to easy escape via suppositional reflections, such as the obvious one that if sufficient control over psi can be acquired for practical application, all resistance to the findings will cease. It is not realistic to build even on such reasonably probable but still uncertain developments. For that matter, the reaction to an ever-so-demonstrable claim can be pitifully slow and reluctant. Look at the history of hypnosis.

What can be gained from a reconsideration of the forces resisting parapsychology? There is something very puzzling in the adherence of so large a body of scientists to a limiting philosophy, holding it as practically an unwritten law of nature, a guiding principle for judging all psychological truth, a framework with which all incoming ideas have to be squared. Such a situation as this in so central a science as psychology is worth having exposed in sharp relief. For this physicalistic concept of man has become an uncontested conviction; something, perhaps, that no one, not even the psychologists themselves, would have suspected without this emphasis which the challenge of psi has given it.

The great service to psychology which this physicalistic assumption has doubtless contributed is the protection it has afforded against a range of unclassifiable, unexplainable, unassimilable experiences. It is a fence on the edge of the jungle that keeps out strange creatures that the psychologists dare not let into the clearing to which they have with difficulty introduced a measure of lawfulness.

But one can now recognize some value, too, in this materialistic formulation for the significance it gives to psi phenomena. In fact, the more strongly and the longer psychologists hold to the formula, the more important they are making psi phenomena and the investigation of them. *What other scientific basis is there for challenging materialism?* What hope is there for rescuing psychology from its unhealthy retreat to the dubious shelters and delusory security of the physical sciences except through the establishment of a clear-cut, nonphysical area of action, a zone of occurrences, of realities, that are not physically explainable but that are the proper domain of psychology?

May one not say, then, that the more difficult a time this "unredeemed" psychologist majority gives the parapsychologist, the more important they are making his part in the restoration of the science of human personality to its rightful preoccupation? Is not this resistant professional group—or rather, the five-sixths of it—turning the parapsychologist's humble inquiries into a mission of vaster significance? The rejection of psi is forcing him into a role of importance which the early acceptance of it would have made unnecessary, the task of saving psychology from its own self-immurement in an untested philosophy of physicalism.

The future historian of science may look back upon this basic issue between psychology and parapsychology as the culmination of nineteenth-century reaction to scientific materialism begun, among others, by the founders of the psychical research societies. This clash may represent a second stage in the Age of Enlightenment that followed the Dark Ages. Thus far the sciences have been very successfully rescuing large areas of problems from supernaturalistic interpretation. But in over-zealously barring out anything that even suggests the supernatural, they have evidently developed a too-narrow conception of the *natural*, one that equates it to the physical—

the physics of space, time, and mass. There is a need now to recover certain areas of unsolved problems from the extremes to which these tendencies have carried modern scientific thought.

Warner's survey has served to remind us not only that we have not gone very far yet toward acceptance in parapsychology, but that in the very vigor of the resistance we can now better appraise the magnitude of the job ahead of us. If we interpret aright, the issue is between psi and an all-physical view of man. Those who know the evidence for psi and can foresee the *eventual* acceptance of it can now the better appreciate its significance in turning the materialistic tide of thought because of the very immensity of the difficulties that are still to be met. In planning, then, for the longer siege, the parapsychologist will be sustained not only by the assurance of eventual victory—even Warner's survey supports that distant hope—but by the promise of its being a greater one by reason of the unexpected strength of the entrenchment of the position he is assailing.

J.B.R.

A TEST OF THE OCCURRENCE OF A PSI EFFECT BETWEEN MAN AND THE CAT[1]

By KARLIS OSIS

ABSTRACT: This paper is a report of a preliminary experiment in a program of research into the problem of whether animals possess ESP ability. The experimental animals used in the tests were six common domesticated cats. For each trial the animal was placed in an enclosed runway containing turns and obstacles, at the end of which were two food cups. It was the task of the agent in the experiment to try to influence the cat to choose that one of the two cups selected as the target.

The experiment consisted of two sections: an exploratory section, in which one person worked alone handling the cats and serving as agent; and the main section, in which one person served as the agent while another handled the cats. The exploratory tests (1700 trials) gave statistically significant results. The total scores of the main series (2200 trials) were at the chance level, but other analyses yielded significant results. A comparison was made between some cats which, it was predicted, would score positively and others which, it was predicted, would score negatively. The difference between the two groups was statistically significant.

Further analyses which showed evidence indicating psi effects were based on comparisons of scoring at different times of day and between the first and last half of the series for each cat, on the chronological distribution in each agent's scoring, and on the tendency of the cats to concentrate their success in the first group of trials of the experiment.

Dr. Osis, who came to this country from Latvia, is one of the few who have obtained an advanced degree based on a thesis in parapsychology. In his case the degree was taken at the University of Munich. Dr. Osis is now a member of the Duke Parapsychology Laboratory.—Ed.

THE QUESTION of whether animals possess ESP ability often arises when we have to explain the surprising orientation achievements of some species. Migration and homing are not fully understood even today and there is still a strong possibility that psi (ESP) operations could be responsible for them (**10**). There have been many experimental investigations of mammals in which the sensory functions apparently could not have provided a sufficient basis for

[1] In the main experimental series reported here, Miss Sally Rhine, Research Assistant in the Parapsychology Laboratory, served as co-experimenter for all but one subdivision, in which Mr. Carl Betz took part. This co-operation is acknowledged with appreciation, as are also the counsel and assistance of Drs. J. B. Rhine and J. G. Pratt. The research was in part supported by the Rockefeller Foundation.

orientation. In some cases of laboratory experimenting, such as Gundlach's experiment with cats and pigeons (**3**), the animals failed in orienting, but in other cases results were obtained that still remain unexplained, especially the work of Krüger and Vogelberg with blind rats and mice (**6**) and that of Neuhaus with mice (**8, 9**). Field experiments with mammals have also given orientation results without any known sensory basis (**2, 4, 5, 7, 14, 15, 16**). A more extensive review of the literature concerning psi in animals is given by J. B. Rhine (**10**) and the reader is referred to it for detailed information.

Although the results of years of research on the animal orientation problem include findings that the known sensory abilities will not explain, they do not give any evidence that would tell definitely just what abilities *are* involved in this behavior (**8**). The psi hypothesis is supported, but it is not established by past work done in this field.

We decided to begin the present work with a somewhat different approach. Instead of looking for indications of psi in the general orientation ability of the animal, we wanted to see if we could get direct evidence of psi in animals. If successful, we hoped that the findings would eventually throw light on the general orientation problem.

Little has been done previously in the way of a direct experimental investigation of psi in animals. The published work consists of Drs. J. B. and L. E. Rhine's tests with a horse (**12, 13**) and of Bechterev's work in which he gave complex telepathic commands to dogs (**1**). The former experiment yielded impressive positive results, and the Rhines concluded that ESP appeared to be the best explanation. Bechterev, in a short series, obtained striking results which convinced him that the dogs had telepathic ability.

It was decided to carry out the present investigation upon small animals that could readily be used in the laboratory. We wanted to keep the situation simple and as close as possible to the test methods for testing psi in humans and also to methods common to animal psychology. Above all, we wanted to take only a safe step and not a large uncertain jump into the unknown. The multiple-choice method was selected as one that the fields of ESP and animal

psychology have in common. This we took in the simplest form, a choice between two dishes of food.

In this research the first formulation of the question regarding psi in animals was as follows: Does psi occur between man and animal? The question of how it might occur had to be left for later work. Naturally, when significant results were obtained, every opportunity was taken to make observations that might throw light on the nature of the factors involved.

Friendly relations between experimenter and subject are considered basic conditions for psi experiments with humans. The animals used in this experiment were cats. Because of the cat's long domestication, it was hoped that friendly relations would be easier to establish with them than with rats, mice, or other small animals. Although the question of ESP in dogs is also of interest because of suggestions from the anecdotal material that they too possess extrasensory ability, tests with dogs would require larger apparatus and larger rooms, and it was not considered necessary to use them for the first experiments. In the tests reported here six common house cats were used; all but one were females. One cat was adult; the age of the others ranged from seven weeks to five months. Kittens were preferred because any habits they formed in the experimental situation that would interfere with ESP responses seemed to be more flexible and easier to break.

The main procedure consisted of the attempt by the agent to influence the hungry cat to go first to the one of two available food dishes that was selected for that trial by the use of a shuffled pack of cards. The work to be reported can be divided into two stages, corresponding to the degree of safeguarding imposed. Stage 1, the exploratory section, consisted of nine series (1700 trials) done with only one person present during the test. He acted as both experimenter and agent. Stage 2, the main experiment, consisted of eleven series (2200 trials) done with two persons present for every trial, the experimenter and the agent (that is, the person who was attempting to influence the kittens). Tables 1 and 2 give the names of the series and the conditions for each.

Apparatus

The apparatus, shown in Figure 1, is the final setup as it was used in the second stage of the experiment. It consisted of a large

T-shaped pathway enclosed in wire netting. This was eight feet long, 10 inches wide and 18 inches high. Two cardboard tubes, 5¾ inches in diameter (T_1 and T_2) were connected to the ends

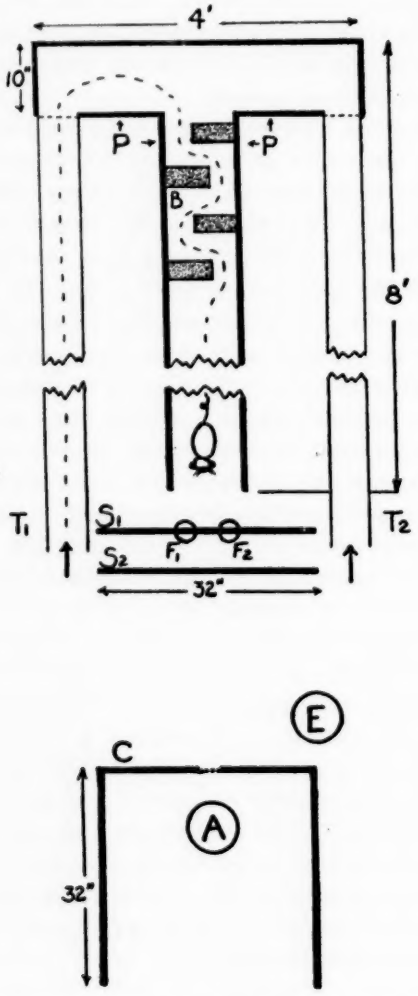

FIG. 1. Experimental apparatus as used in the main experimental series.

of the T, running parallel to the center wire net pathway (P). The animal was introduced into the open end of one of these tubes and it ran up to the top of the T and then down the center pathway. In the pathway were placed brick barriers (B) that were changed frequently to keep the situation a novel one for the animals. At the end of the pathway was a double presswood screen (S_1 and S_2). The part of the screen nearer the kitten as he approached S_1 was 42 inches high and 32 inches wide, with an opening four inches high beneath. The other part, S_2, had no opening but rested on the ground six inches nearer the experimenter or agent and was 10 inches high. Two metal cups, each containing a little food (F_1 and F_2), were placed in the opening under S_1. On each trial one of these cups served as the target. In the first stage the experimenter was located at position A in Figure 1. In the second stage of the experiment a cubicle (C) for the agent, who was attempting to influence the kitten, was added. This had three walls and a ceiling made of plywood, with a window of dark blue plastic (Audograph record material) on the front wall to enable the agent to see the cups under the screen. Because of the darkness of the interior of the cubicle, this served as a one-way screen and the cat could not see the agent. The open side of the cubicle was close to the wall of the experimental room. The positions of the agent and experimenter in the second stage of the experiments are shown by A and E respectively.

Other parts of the set-up are features that were used in only a part of the experimental series, and they will be described elsewhere.

Procedure

Many variations of the procedure were used, but all the series had the same basic features.

The agent was isolated from the kitten, being back of the screen in the first stage and in the cubicle in the second stage. He had a deck of playing cards that had been randomly arranged by an assistant according to Tippett's table of random numbers. The person who arranged the cards was not otherwise involved in the experiments. The agent always took the top card to indicate whether the target would be the left or the right cup under the screen. The color

of the card indicated which cup would be the target, black indicating the one on the agent's right and red, that on his left.

In the exploratory, first stage, the experimenter worked alone. He placed the food in the two cups, arranged them under the screen, and introduced the kitten into the tube before he looked at the card to see which cup was to be the target of that trial.

In the more advanced, second stage, the experimenter was outside the cubicle and did not know the target until the trial was over. He arranged the cups, handled the kitten, and recorded the results. The detailed order of events was as follows: The agent looked at the card inside the cubicle, then put it face-down at a designated place where the experimenter could see only one corner of the back of it. The experimenter definitely could not see the color of the face of the card. After the card was placed, the experimenter put equal amounts of food in each cup and placed them under the screen. He then introduced the kitten into the tube, T_1 or T_2, systematically changing sides from trial to trial.

Usually the kitten ran quickly through the approach path, jumping over or going around the bricks. The kittens then came toward the cups and went to eat first at one cup, then the other cup. Both E and A kept silent while the kitten was running. The cup selected first was judged to be the kitten's response, even in the few cases in which the kitten did not actually eat the food from the cup. The kitten was allowed to eat from both cups every time regardless of whether the response was "correct" or "incorrect."

Ten trials were counted as a run and 20 runs as a series.

In the first stage, the experimenter (who was also the agent) recorded both the target and the kitten's response. In the second stage, E first observed and recorded the kitten's choice, and only then did he take the target card from the corner where it had been placed by A and record it also. E then prepared for the next trial while A looked at the next card and placed it face down in the designated place.

The kitten was not consciously handled in any different fashion when he made a correct response and when he made an incorrect response. The attempt was made to be equally friendly in both cases in order to keep the kitten ignorant about the task it was to

perform and of how successful it had been. Keeping the cat from getting any information about how its responses pleased or displeased us was thought to be an important safeguard against the use of any sensory information the set-up might conceivably afford.

When a kitten developed a habit to go first on every trial to one cup (for example, the one on the right), E tried to break the habit by shifting the cups a short distance toward the preferred side to put the non-favored one in a position closer to the kitten as it approached the two cups. The aim was to shift the cups just far enough to present the kitten with a real choice situation, as indicated by its stopping for a moment and showing signs of indecision. Naturally, we should expect that the weak psi influence would be more effective in this case than in the situation where there was a strong preference for one side.

It is very important to note that E did not know what the target would be when he shifted the cups. Obviously, if he did know it would be an easy task to place the cups in a favorable position.

At times this method of breaking the side habits was ineffective and the experiment was interrupted while the kitten was trained to go to the non-favored side by placing food only in that one cup. On some occasions 18 trials were required to break the habit the first time. This indicates that the habits were in some cases so strong that we could not suppose a slight influence like psi could break them. These training trials were actually very few in number. They were recorded as separate observations and the experiment proper was continued only on the next day.

In two of the exploratory series and in three of the main series we introduced different conditions in the pathway to cause unpleasant interruptions or distractions for the kitten. Wind was blown in its face by fans, a bright light was suddenly switched on, or an electric shock was administered from an induction coil. Control runs under normal conditions were alternated with every experimental run under unpleasant conditions. In one series the drug D-Amphetamine Phosphate in doses of 1.25 mg. was used in the experimental runs, and it was necessary in this instance to have the control runs precede the experimental runs or follow on the next day.

Relatively early in the work the experimenter gained the impres-

sion that novelty in the situation helped prevent habit formation and thus offered more opportunity for a psi effect. Care was taken, therefore, to change the kinds and shapes of the barriers in the approach path, the distance between the cups, and, in the latter part of the investigation, the types of foods.

General Results

Exploratory Series

The first step of the investigation was the exploratory work in which the apparatus and the procedures were developed. All the runs of the exploratory series were made with only one observer (K.O.) who served as the agent. Essentially the same apparatus was employed for all the series, except for a preliminary period (the results of which are considered separately) and three series of the exploratory stage. The conditions for these series will be described in turn.

Most of the first exploratory experiments were made with the kitten Judy, who seemed to show more extrachance effect than any other kitten used.[2] In Judy's first series in the exploratory stage

Table 1

Results from Exploratory Period

Kitten	Series	Conditions	N	Deviation	CR	P
Judy	I	Beginning; 2 foods	200	+19		
Judy	II	Two balls	200	+22	+41	
Judy	III	Food in 1 of 2 cups	200	+ 6		
Judy	IV	Two foods; wind in path	200	− 6	2.90	.004
Mika	I	Beginning; 2 foods	200	+13		
Mika	II	Food in 1 of 2 cups	100	+ 2	+15	
Timmy	I	Beginning; 2 foods	200	+18		
Timmy	II	Two foods; wind in path	200	− 1	+17	
Bobo	I	Beginning; 2 foods	200	− 1	− 1	
Total	.....	.....	1700		+72	3.49	.0005

[2] Judy did 100 preliminary trials without any enclosed pathway or screen; an assistant simply released the kitten three yards from the target cups, which were on the floor a short distance from the agent's feet. In these 100 trials Judy scored 60 hits or 10 above chance expectation, which is, of course, 50%. The result is not included in the total results for the exploratory period, however, because at this early stage the screen which concealed the agent had not been set up.

(see Table 1) the apparatus and screen were used except for the middle path, P. The experimenter was the agent and handled the kitten. The kitten was placed in one of the tubes from behind the screen and she emerged from the other end to run back over the open floor toward the cups. Judy scored 119 hits in 200 trials, or a deviation of +19, which is almost the same rate as in the first unscreened series. It appears, therefore, that the safeguards introduced in this series did not adversely affect the results.

In Judy's second series two small rubber balls were used as targets instead of food cups. The balls hung down on elastic rubber strings behind the screen. The kitten, approaching through the path, could see the balls in the four-inch opening under the screen and she attempted to catch one. The strings were tied securely to a rod that was fastened parallel to the screen on a horizontal axis. For every trial the rod was turned back and forth by hand and this moved the balls. Judy scored +22 in this series (200 trials). I consider this set-up to be one favorable for psi influence, because at every trial the kitten stopped and observed the moving balls, and then jumped at one. However, this set-up was used only with Judy, for the other kittens made no response to the balls.

The adult cat, Mika, made her first series without any tubes and enclosed pathway, but with a screen in front of the agent. The cat was released by an assistant as previously described. Mika scored +13 in the first 100 trials of her first series, and she remained on the chance level for all other experiments with her. There was no change of external condition within the series to explain the change in scoring.

In all other exploratory series the apparatus as shown in Figure 1 was used except that the experimenter worked alone without a cubicle.[3] In the last two exploratory series an assistant handled the kittens. In Series III for Judy and Series II for Mika, food was placed in only one cup and there was a small screen between the cups. The assistant knew where the food was and possibly also perhaps acted as agent involuntarily. The total deviation for both kittens was only +8 for the 300 trials.

[3] The cubicle, as well as the second experimenter, was actually introduced during a fraction of the last two exploratory series. In order not to divide the series, however, these trials were counted as falling within the Exploratory section.

In general, good results were obtained at the beginning of the experiments. The first 900 trials with four kittens yielded a total deviation of +89, which is highly significant (CR = 5.93, P = .000,000,004).

Then there was a remarkable drop in scoring. With Judy the drop might be explained either by a serious illness that developed or by her increased friendly relations with the other kittens. (During the good scoring period, she was hostile to other cats and very friendly with the experimenter and his family.) With the other kittens there was no explanation from the external conditions for such a sudden decline.

The total deviation for all the exploratory work is +72 in 1700 trials, with a CR of 3.49. These results, which are summarized in Table 1 by individual kittens and for each condition used, are statistically significant. From these data we took as a working hypothesis the view that psi interactions between man and cat do occur, and we were encouraged to undertake further experimental work with better experimental conditions. Naturally the exploratory results were not considered adequate for reaching conclusions regarding psi in kittens.

Series for Predicted Negative Scores

In the exploratory period the scores of the animals declined to such a degree that it seemed unlikely that these kittens would give significant positive deviations in further tests. We therefore attempted to find unpleasant conditions for these subjects to see if such conditions would cause them to avoid the target. Negative deviations from chance expectation would indicate avoidance of the cup about which the agent was thinking, and thus would supply us with the same degree of evidence as comparable positive deviations.

Missing the target more often than would be expected by chance is a frequently observed occurrence in all types of psi experiments. This type of evidence has been summarized by Rhine under the term "psi-missing" (11). I do not know of any special investigation of animal psychology concerning the psi-missing effect, but the frequency of this effect in human beings suggested that it might be worth while to test the effect of unpleasant conditions with the animals. Further encouragement was derived from the fact that

Krüger and Vogelberg, in their carefully conducted orientation experiments with mice (6), found their best results in the negative direction.

In the last two exploratory series made with Judy and Timmy we therefore attempted to get negative deviations. Powerful fans were placed on both sides of the path P, one yard before and on each side of the targets so that they blew in the face of the approaching kitten. The kittens showed hesitation when approaching the fans; they lingered, sometimes turned back, turned their ears backwards, partly closed their eyes, and held the tail erect and moved only the tip of the tail as is characteristic of annoyance. Ten trials made with fans constituted the experimental run and ten trials without fans was the control run. The order of the control and experimental runs was varied.

The cat Judy scored −1 in 100 trials with the fans and −5 in 100 control trials. Timmy in the same number of trials scored +1 with the fans and −2 without. These results were disappointingly near chance expectation. They have been added in with the other exploratory trials in Table 1.

Main Series: Conditions for Predicted Negative Scores

The experiment was continued into the second stage and an independent experimenter was now introduced. This plan allowed for an observer located outside the cubicle while the agent was seated within. For the most part, the second experimenter was Miss Sally Rhine (S.R.), who alternated as agent and observer with the author (K.O.). Two hundred trials were done with Mr. Carl Betz alternating as second experimenter with K.O.

The cubicle itself, of course, had the effect of isolating the agent from the kitten and thus removed the possibility of guidance by sensory cues. At first, the conditions for producing negative scoring were tried further. Again the kittens disappointed us. There were no differences between the scoring in the experimental (unpleasant) and the control runs. In general, the results were somewhat negative under both conditions involved in the comparison. It seems that the kittens may have been associating the unpleasant conditions with the experiment as a whole instead of responding differently to the particular types of runs, experimental and control.

Table 2

TESTS WITH TWO EXPERIMENTERS SHOWING RESULTS FROM PREDICTED LOW- AND HIGH-SCORING KITTENS

Kitten Series	Conditions*	N	Deviation	Diff. High-Low	CR_d	P
LOW-SCORING GROUP						
Judy V.....	A	200	−12			
Timmy III.....	A	200	− 7			
Timmy IV.....	B	200	+ 2			
Timmy V.....	C	200	−11			
Total.....	.....	800	−28			
HIGH-SCORING GROUP						
Chippy I.....	D	200	+ 6			
Chippy II.....	E_1	200	+ 4			
Chippy III.....	E_2	200	+ 9			
Vabuly I.....	D	200	+ 3			
Vabuly II.....	E	200	− 7			
Susie I.....	E_1	200	+ 3			
Susie II.....	E_2	200	+ 7			
Total.....	.....	1400	+25	53	2.38	.017

*The general conditions of different series were:

A: Experimental runs with disturbing conditions (fans and bright lights) alternated with control runs.
B: No control runs, but with disturbing conditions (fans, lights, electric shock, water) in different combinations.
C: Kitten received 1.25 mg. per run of D—Amphetamine Phosphate.
D: No disturbing conditions.
E: No disturbing conditions, but with frequent changes of preferred foods. The numeral subscripts indicate first and second series with the same kitten.

After 400 trials, therefore, it was decided to continue without the control runs. In the next series (Timmy IV in Table 2) new unpleasant conditions, such as electric shock, a water pan in the path, or a 300-watt light directly over the path with a mirror under it on the floor, were tried in various combinations. We did not wish to study the effect of the different types of disturbances, but rather to discover whether the scores could be lowered when several unpleasant conditions were present.

However, we were again disappointed. Judy died from an illness before the series was begun, and Timmy scored only +2 in 200 trials.

The general impression from the use of these disturbing condi-

tions, particularly the electric shock, was that the effect upon the kitten was such as to inhibit any psi function. We therefore decided, finally, to use kittens that seemed to show a tendency to score negatively and see whether this direction of deviation would be accentuated by the use of a stimulating drug. For this purpose, a series was run with Timmy (V) in which the stimulating drug D-Amphetamine Phosphate was given in doses of 1.25 mg. for each experimental run of 20 trials. The score for 200 trials was −11.

The results for the 800 "predicted negative" trials in the second stage of the experiment, summarized in the upper part of Table 2, gave a deviation of −28. This is suggestive of a real negative effect, with a CR of 1.98 (P = .05).

Main Series: Predicted Positive Scores

Three new kittens, Chippy, Vabuly, and Susie, were introduced during this second or experimental period. The work of the exploratory period indicated that new kittens start out with a positive deviation. Hoping that this would occur in the experimental period with these three new kittens, we did not employ any negative conditions with them. Instead, we tried to introduce pleasant novelties in some series. In the first series of 200 trials with Chippy, no special conditions were used, and the score was +6. In two further series, a variety of foods were used to maintain the strength of motivation, and these 400 trials scored +13. Similarly, Vabuly and Susie scored slightly above chance expectation in 800 trials with the same conditions. The total results for the "predicted positive" scoring group, shown in the lower half of Table 2, were 1400 trials with a deviation of +25, which is not independently significant.

Results from Further Analyses of the Data

The total scores of the second stage with the two experimenters were not significant, but further analyses of the data yielded evidence of several different kinds indicating the presence of a psi effect.

Difference between the Two Groups

The first such indication was the difference between the predicted low- and high-scoring kittens. There is a difference of 53 successes between them. The CR of the difference is 2.38, which gives P = .017 for a difference as large as this in either direction, or .009 if

we take into account that the results in the two groups follow the predictions. Only one series in the low-scoring group was positive and only one in the high-scoring group was negative, which shows consistency in the tendency to score above or below the expected chance level in these groups.

As already stated, S.R. and K.O. alternated in serving as agent,[4] and several interesting results were found to be associated with this fact. Of the difference between the two groups, 35 points of the total of 53 were made with K.O. as agent ($CR_d = 2.31$, $P = .021$), 13 with S.R. as agent ($CR_d = .85$, $P = .4$) and the remaining five points with Mr. Betz, who did 100 trials. This difference between agents is not significant, but it is suggestive and seems logically easy to interpret when we understand that K.O. was the one chiefly conducting the experiments and was the person who divided the two groups on the basis of his experience with the kittens in the exploratory stage. Actually, the other experimenter, S.R., did not feel the same degree of confidence that she could influence the animals to score below chance.

Scoring Distribution Within the Series and for the Time of Day

In the exploratory stage it was observed that each kitten's score declined as his work progressed. This impression was tested by dividing the data for each subject at the mid-point and pooling each half for all the kittens. The score for the first half was found to be significantly higher than that for the second half, the CR_d being 4.07 (see Table 3).

In that stage, the experimenter worked chiefly in the early forenoon and only occasionally at night, as he thought those were the best times of day for both himself and the kittens. The first stage therefore offered no good opportunity to test differences between favorable and unfavorable times of day.

The work of the second stage was done in both the forenoon and the afternoon (but not at night), and the data could therefore

[4] In two series 10 trials more were done by K.O. than by S.R. In all instances in which an analysis involves a comparison of these two agents, the trials and hits have been adjusted slightly on an arbitrary basis to equate the trials for purposes of convenience. The extra 10 trials by K.O. were omitted, thus reducing his score by 7 hits; 10 trials and 5 hits were added to S.R.'s data. The correction obviously could not have appreciably affected the results.

Table 3

CHRONOLOGICAL DISTRIBUTION OF SCORING, ACCORDING TO KITTEN, DURING EXPLORATORY PERIOD

Kitten	Total Trials	First Half	Second Half
Judy	800	+41	0
Timmy	400	+18	−1
Mika	300	+12	+3
Bobo	200	+ 7	−8
Total	1700	+78	−6

$CR_d = 4.07$

be studied for declines during the day as well as during the series as a whole. For the latter purpose, each series of 200 trials was divided into halves. The reason for dividing each series in half instead of the work of each kitten as a whole was that the conditions were changed at the start of each new series on the assumption that the novelty would cause high scoring to be resumed. There was a significant decline from the first to the second half of the pooled series ($CR_d = 2.35$; $P = .019$). (See Table 4.)

Table 4

CHRONOLOGICAL DISTRIBUTION OF RESULTS OF MAIN SERIES IN RELATION TO TIME OF DAY

DATA OF INDIVIDUAL SERIES, POOLED

Time of working during day		First Half	Second Half	Total
	A.M.	I 704:+20	III 590:+13	1294:+33
	P.M.	II 396:+ 6	IV 510:−42	906:−36
	Total	1100:+26	1100:−29	2200:− 3

Differences

A.M.—P.M. (Total) = 69

$CR_d = 3.01$

$P < .0026$

1st half—2nd half = 55

$CR_d = 2.35$

$P = .019$

The comparison of A.M. and P.M. work also showed a decline. The deviation of the afternoon runs was negative and on the average a little larger than the positive deviation of the forenoon. The scoring difference of the two times of day was highly significant ($CR_d = 3.01$; $P < .0026$).

It is interesting to inquire how these two declines interact, and the data have been presented in Table 4 to show the relation between the two. The distribution in terms of the four intersections (quarters) presents no real surprises, but it emphasizes the extent to which the negative scoring is concentrated in the part of the work that was done both in the second half of each series and in the afternoon. The data in the fourth cell of Table 4 have a CR of 3.72 ($P = .0002$). It is also interesting to observe that the work with K.O. as agent was primarily responsible for these results, as it produced a deviation of −27.5 in 287 "second half—P.M." trials and by itself gives a P of .001. In the afternoon session K.O. felt fatigued, but S.R. did not.

Chronological Decline with Each Agent

Further study of the chronological distribution within the 200-trial series in relation to the work of the two agents shows that there is a striking similarity of decline between S.R. and K.O. The ten series in which S.R. and K.O. participated equally as agents were compared, and the work of each agent in each series was divided into chronological quarters. The results are shown in Table 5. (The 200 trials in which a different agent alternated with K.O. have been omitted from this comparison.) In the first, second and fourth quarters both agents' scores are practically the same. The regression coefficients indicate that there is a very consistent relation between the score and the order of the quarters for K.O., a suggestive one for S.R., and that both declines are jointly related to a significant degree to the order of the quarters in the series.

When the chronological decline is considered further in terms of the 10 runs of the pooled series (keeping agents separated), the only significant deviation occurs in the last run. This run gave a deviation of −21.5, which is twice as much as the next largest deviation, +10.5, which was scored on both the second and the third runs. The deviation on the tenth run gives a CR of 2.90 with

Table 5

CHRONOLOGICAL DISTRIBUTION WITHIN MAIN SERIES IN RELATION TO TWO PRINCIPAL AGENTS

DATA OF INDIVIDUAL SERIES, POOLED

Agent	Quarters			
	First	Second	Third	Fourth
S.R.	250:+8	250:+3	250:+2	250:−14
K.O.	250:+8	250: 0	250:−8	250:−13
Total	500:+16	500:+3	500:−6	500:−27

F-values for analysis of variance based on regression coefficients (d. f. = 2, 1):
S.R. 9.29 P=< .1
K.O. 187.22 P=< .01

Total 66.13 P=< .02

$P < .004$. Naturally, this figure selected out of ten adds nothing in the way of evidence for psi, but it indicates that the decline to a point *below chance expectation* is a common feature of the results. In all the analyses applied to sections of the data, the largest deviations are negative, coming either from negative scoring kittens or from work done in the afternoon or at the end of some larger chronological unit.

Primacy Effect

A factor that contributed somewhat to the chronological declines is the primacy effect, that is, the tendency for the best scoring to occur in the kittens' first trials. For comparison, the figures on this effect from the exploratory period are included in the analysis. Only the results of the preliminary tests with the first kitten, Judy, are not presented, and this is due to the fact that she began with various informal conditions in the first 100 trials without any tubes, pathway, and screen, and these results cannot reasonably be compared with the others. This leaves three kittens from each stage, and as a measure of the primacy effect the results of the first 100 trials were compared with the second 100 trials from each animal. The reason for using each kitten's second 100 trials for comparison with its first 100 trials was that one of the kittens (Bobo) made a total of only 200 trials. The results, shown in detail in Table 6, indicate the

same strength of primacy effect in the two stages of the experiment, each one showing a strong positive score in the first trials and a significant difference between the two 100-trial groups. (For each stage, $CR_d = 2.93$; $P = .0034$.) Thus the concentration of high scores in the first trials as suggested in the exploratory stage was confirmed in the main experiment.

Table 6

PRIMACY EFFECT IN DATA

EXPLORATORY PERIOD

Kitten	Series	First 100 Trials	Second 100 Trials	Diff.	CR_d	P
Timmy	I	+13	+ 5	8		
Mika	I	+13	0	13		
Bobo	I	+ 7	− 8	15		
Total	.....	+33	− 3	36	2.93	.0034

MAIN EXPERIMENTAL PERIOD

Kitten	Series	First 100 Trials	Second 100 Trials	Diff.	CR_d	P
Vabuly	I	+ 7	− 4	11		
Chippy	I	+ 3	+ 3	0		
Susie	I	+14	−11	25		
Total	.....	+24	−12	36	2.93	.0034

Results with the Outstanding Kitten of the Main Experiment

The results with one kitten, Susie, shown in Table 2, contributed in an unusual degree to the various effects that were brought to light by the foregoing analyses. It seems worth while, therefore, to present the work with this kitten separately because the stronger concentration of the various effects in her data might offer some further clue regarding the psychological principles involved.

The chronological distribution of Susie's results for each of her two series is shown in Table 7. These results are given in terms of the two halves of the series, as they were considered previously for the data as a whole, and also in terms of the quarters of the series. Examination of the deviations shown in Table 7 reveals that the decline for this subject was a remarkably consistent one, extending

Table 7

CHRONOLOGICAL DISTRIBUTION OF SUSIE'S RESULTS

Series	By Halves		By Quarters			
	First	Second	First	Second	Third	Fourth
Susie I.....	100:+14	100:−11	50:+ 9	50:+5	50:−2	50:− 9
Susie II.....	100:+ 7	100: 0	50:+ 4	50:+3	50:+1	50:− 1
Total.....	200:+21	200:−11	100:+13	100:+8	100:−1	100:−10

Diff. = 32
$CR_d = 3.20$
$P = .0014$

χ^2 test of distribution of hits and misses among 4 totals:
$\chi^2 = 12.40$ (3 d.f.)
$P = .006$

in an unbroken manner throughout the four quarters of each series. It is interesting to observe that the decline is significant for Susie's work alone, though this is, of course, not offered as evidence independent of that based upon the work of all the subjects.

A further effect found in Susie's results may be mentioned, though this was characteristic of her performance alone. This effect is a general incline in the scoring level through the 10-trial run. The results for the run distribution have been shown in Table 8 for each series singly and for the two combined in a manner that indicates

Table 8

DISTRIBUTION OF SUSIE'S RESULTS WITHIN THE RUN IN RELATION TO THE CHRONOLOGICAL DISTRIBUTION

		First Half of Series	Second Half of Series	Total
First Half of Run	Susie I	I. 50:+ 1	III. 50:−13	100:−12
	Susie II	50:+ 8	50:− 1	100:+ 7
	Total	100:+ 9	100:−14	200:− 5
Second Half of Run	Susie I	II. 50:+13	IV. 50:+ 2	100:+15
	Susie II	50:− 1	50:+ 1	100: 0
	Total	100:+12	100:+ 3	200:+15

χ^2 test of distribution of hits and misses among totals of 4 quarters:
$\chi^2 = 16.24$ (3 d.f.)
$P = .001$

how this effect was distributed over the two halves of the series. It may be seen from the results that the incline within the run was characteristic only of the first of Susie's series, for which there was a difference of 12 hits between the first and second halves of the run in the first half of the series and a difference of 15 for the second half of the series. For her work as a whole, the two halves of the run showed an incline giving a difference of 20 hits, which is of suggestive value only (CR = 2.0; P = .05). The chi-square test of the distribution within the four cells of Table 8 (the interaction of the two halves of the series and the two parts of the run) give adequate support to the conclusion that these are not chance results, though of course this conclusion has already been indicated by the previous analyses, and the present selection is not made primarily with the purpose of contributing further evidence bearing on the question of chance.

Discussion

The experiment was concerned with the problem of psi between man and cat. The work was merely exploratory for other related problems. The main question for discussion, therefore, is whether the results are explicable without psi interactions between man and animal.

The first consideration, naturally, is whether the results may be ascribed to chance variation. The total deviation of the second stage or main experiment is not significant; but there are significant effects in the data from this section that were expected on the basis of the exploratory series. These are independently significant and such as to exclude the chance hypothesis. Also, there are other measures which only approach significance but which support the evidence.

After the statistical significance of the results is established, it is necessary to consider whether the results could be explained on the basis of sensory cues. A general impression from all the data is that there was no learning effect such as would be expected if the kittens followed some sensory cues to make their orientation toward the target. If they were using sensory cues they should improve as the experiment continued. There was no indication of this: rather, the observed primacy effect was contradictory to the learning expected where cues are used. The conditions afforded no occasion for using

cues, since the kittens did not know which cup was the target. They always ate at both cups, and we attempted to treat them the same when they made an incorrect choice as when they made a correct one.

Visual cues from the agent were excluded by having him in the cubicle. There was no opportunity for the cat to see any of the agent's movements. The cubicle also excluded any possible influence of the agent's breathing and of any slight sound signals which he might have made unintentionally. It is clear from the results that the introduction of the cubicle did not lead to any reduction in the amount the score deviated from mean chance expectation. In the last two series of the exploratory stage during which the cubicle was introduced, the average deviation per run before the cubicle was used was .29, and after its use, .39; and in the first two series of the main experimental period with the cubicle, the average run deviation increased to .47.

The odor of food in the two cups could not give any clue regarding the target for the food was placed in both cups by the experimenter while he was ignorant of which would be the target.

The experimenter who was outside the cubicle did not know which cup was the target until after the trial was made, so he could not have provided any sensory guidance for the kittens. He alone prepared and placed the cups and handled the kittens. He also did the recording. The possibility of recording errors was excluded by the fact that when he recorded the response, he was ignorant of the target and the random arrangement of the targets prevented his reasoning what the next target would be.

Is it possible that the results were influenced by the experimenter through his shifting of the cups on the basis of an ESP impression of the next target? That this counterhypothesis to psi interaction will not apply to all the results is shown by the fact that the most significant scores were obtained at the very beginning of each cat's period of testing, and at this time there was little or no need for shifting the cups. This was done only after enough trials for the animal to acquire a side preference and for the experimenter to observe what the position habit was. There are also reasons for believing that the experimenter did not use ESP in shifting the cups even in later stages of each cat's work. If this had happened, we

might expect that the results obtained would be those desired by the experimenter. The fact is, however, that most of the results were unexpected and occasioned disappointment at the time. Since this "ESP of the experimenter" hypothesis will not explain the primary effect, it seems unlikely that it was a factor of any importance in any part of the investigation.

This has been throughout, in a very real sense, an exploratory investigation that has been concerned with one and only one question, whether any evidence could be found of a psi effect in a relationship between man and cat. This is the question with which it was necessary to deal before any effort could be made to investigate the nature of a supposed psi effect in this situation. There is no point in considering problems regarding the nature of such an effect, the respective roles in it of experimenter and kitten, and the like, unless we first have definite evidence that psi may play some part in the relationship. There was nothing in the design of the experiment, therefore, that was intended to deal with questions that properly belong only to a later stage of development of the results. Once it was found that the experiment had yielded significant results indicating the presence of a psi effect, however, we then inquired whether any of the further analyses of the data threw any light upon its nature.

In general, the results of the present investigation do not provide answers to the questions that arise beyond the one of psi interaction between man and cat, which has been our main concern throughout the present study. There were some scoring patterns (for example, the contrast between the two agents with respect to their morning and afternoon work) which indicate that the agents were taking an active part in the psi effects; and there were consistencies of performance in both stages of the experiment which suggest that the effects were at least partially focused within the individual cats. The one general conclusion that can be reached on the basis of all the results, therefore, is that the findings cannot be interpreted except in terms of the operation of psi or ESP in the total situation involving both the experimenter and the kittens. This is enough to permit us to say that the kitten was playing some essential part in this relationship and therefore was involved in a basic way in the

psi effect demonstrated. Further work will be necessary and is already under way to show whether evidence can be found for an ESP effect in kittens as well as in other animals under experimental conditions that exclude the experimenter as a direct contributing factor.

The general acceptance by science of the hypothesis that animals possess psi abilities would have such revolutionary consequences that we should have more and better evidence than is ordinarily required before we conclude that the hypothesis has been established. We may say, therefore, that the work presented in this report contains a good amount of evidence that psi occurs between man and cat. The final judgment should not be based upon the investigations of one experimenter nor upon those carried out with one apparatus. It is necessary to work upon the problem with different methods and to have the findings confirmed by different people. The present results lend to the hypothesis as strong support as could have been hoped from a single investigation dealing with this problem.

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SOME PHILOSOPHICAL PERSPECTIVES FOR PARAPSYCHOLOGY

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THREE British philosophers have recently made contributions which deserve the attention of all who are interested in parapsychology. I refer to Professor H. H. Price's paper "Mind Over Mind and Mind Over Matter," which was published in two parts in July and September, 1949, in a little-known and now defunct periodical, *Enquiry;* Professor C. D. Broad's paper "The Relevance of Psychical Research to Philosophy," in *Philosophy,* October, 1949; and Mr. A. G. N. Flew's two talks on the B.B.C.'s Third Programme on the Significance of Parapsychology, reprinted in *The Listener* on September 27 and October 4, 1951. I shall present what I take to be the central theses of each of these contributions, making occasional comments in passing, but postponing the critical discussion until the exposition of all three papers is completed.

Professor Price's main thesis is to show that although, on the face of it, telepathy and PK are utterly different, it is possible to subsume them under a single principle. Price starts by suggesting that the greatest obstacle to theoretical progress in parapsychology is the assumption that the individual mind is an absolutely separate and indivisible unit—a substance as Descartes conceived it in his famous dualism. We may replace this theory with one in which human nature is tripartite, comprising body, mind, and spirit. "Spirit," says Price, is to mean "the fundamental 'I,' the pure ego, the *Atman* of the Hindu philosophers." The psychological sciences are concerned only with the mind. Discussion of the spirit (if anything *can* be said about it) belongs to philosophy. Descartes was wrong in conceiving the mind as an absolutely unitary being. "The facts of abnormal psychology show that *internally* the unity of the mind is unstable and precarious. . . . The facts of psychical research show that *externally* also the unity of the mind is a matter of de-

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gree . . . and that two minds may overlap." Price takes as the unit for psychological thinking not the mind, but the *idea.* This term is not defined. Price says, "I shall assume that its meaning is sufficiently well understood." Minds, and also such things as Freudian complexes and ghosts can be regarded as structures, whose elements are ideas, though such different kinds of structure would differ greatly in complexity, stability, etc.

Let us suppose (1) that once an idea has come into existence it "has, so to speak, an independent life of its own," and (2) that "every idea has an inherent tendency to realize itself in physical form." This tendency would be partially fulfilled when an idea manifests itself as a mental image, more completely fulfilled when it manifests itself as a hallucination, but completely fulfilled only when it causes a rearrangement of material particles. Ideas normally get materialized by way of efferent nerves and muscles. But this, if PK is a genuine fact, is only a special case of something wider. Price suggests that we think of PK in terms of the concept of ideo-motor action. "PK is ideo-motor operation occurring apart from the nervous system and muscles instead of by means of them."

Price now turns to telepathy, which he says seems to him to show "that there is a common unconscious, that at the unconscious level there is no sharp distinction between one mind and another." An idea belongs to an individual mind rather than to the common unconscious if and only if it is more likely to emerge in that person's consciousness than in anyone else's. We are to suppose that every idea has an inherent tendency to cross the threshold of consciousness, no matter whose. A person's threshold of consciousness is conceived as a barrier which tends to exclude all ideas except a few of those which appeal to his dominant interests at the time. Telepathy is comparatively rare because ideas which do not belong to a person's mind find greater difficulty in crossing the threshold than those which do. On this view the threshold is a protective mechanism which is indispensable to the maintenance of one's personal identity.

Price now asks whether we can establish any *link* between telepathy and PK. First, he points out that there are intermediate cases where a telepathically received idea expresses itself in bodily be-

havior; for example, in automatic writing. Another example would be "absent" psychic healing, if this is a genuine fact. This brings Price to his main point. He has postulated that every idea has two tendencies: (a) to get itself embodied in matter; (b) to emerge into the consciousness of any person whose threshold it can cross.

Price suggests three ways of providing a unifying theory:

1. By treating tendency (b) as a special case of tendency (a) on the grounds that, by getting into someone's consciousness, an idea has a much better chance of becoming realized in physical form. "This amounts roughly to saying that PK is the fundamental phenomenon and telepathy is a partial and incomplete form of it, incomplete until the telepathically received idea is realized in action."

2. By treating tendency (a) as a special case of tendency (b), on the grounds that "if an idea cannot get directly into consciousness, it may still manage to do it indirectly [that is, via the sense organs] by embodying itself in the physical environment." On this view PK phenomena should be considered as symbolic, as vehicles for conveying information.

3. Tendencies (a) and (b) may both be derivative from something more fundamental, and this, Price suggests, might be described as a tendency of ideas to *express* themselves. Price does not define this use of "express." However, he goes on to say: "Expression, I think, is always expression to someone, to some consciousness." In view of this, it is not clear to me how, if at all, alternative (3) is supposed to differ from alternative (2). (Incidentally, alternative 1 seems to me more plausible than alternative 2; if we consider PK experiments with dice, it seems difficult to regard the dice-falls as "vehicles for conveying information." What information and to whom? Alternative 2 *may* be more readily applicable to spontaneous cases of PK, but the dearth of well-attested cases makes this difficult to decide.) Price closes with a brief acknowledgment that this theory of PK and telepathy will not account for clairvoyance. "Clairvoyance is something radically different . . . It cannot be explained as a way in which ideas *operate*. . . . it is a way in which ideas are *acquired*."

I turn now to Professor Broad's paper. Broad's main purpose is to show why the findings of parapsychology constitute a challenge

to philosophers. There are, he says, "certain limiting principles which we unhesitatingly take for granted as the framework within which all our practical activities and our scientific theories are confined." He calls these *Basic Limiting Principles.* These principles are of crucial importance in connection with the philosophers' tasks of *synopsis and synthesis.* "Synopsis" is defined as the deliberate viewing-together of aspects of human experience which are generally kept apart by the plain man and even the scientist or scholar; "synthesis" is defined as the attempt to supply a coherent set of concepts and principles which shall cover satisfactorily all regions of fact which have been viewed synoptically. Broad concludes that the "speculative philosopher who is honest and competent will want to widen his synopsis so as to include these facts [that is, the "paranormal" ones]; and he will want to revise his fundamental concepts and basic limiting principles in such a way as to include the old and the new facts in a single coherent system." I think that here Broad hits the nail right on the head.

Most of Broad's paper is devoted to a detailed discussion of basic limiting principles, with which the findings of parapsychology, and these facts alone, seem to conflict. Broad describes nine such principles and groups these under four heads: (1) General Principles of Causation; (2) Limitations of the Action of Mind on Matter; (3) Dependence of Mind on Brain; and (4) Limitations on Ways of Acquiring Knowledge. He does not discuss the logical relations between the principles in question, some of which seem to be derivable from others. Moreover, insofar as principles (2) and (3) presuppose psychophysical dualism which is eschewed by many biologists and psychologists, they cannot be described as part of "the framework within which all our practical activities and our scientific theories are confined." It would, however, lead me too far afield to quote and discuss in detail Broad's account of basic limiting principles. My present concern is with the philosophical perspective which Broad, in summing up, recommends for parapsychology:

> I have the impression that we should do well to consider much more seriously than we have hitherto been inclined to do the type of theory which Bergson put forward in connection with *normal* memory and sense-perception. The suggestion is that the func-

> tion of the brain and nervous system and sense-organs is in the main *eliminative* and not productive. Each person is at each moment capable of remembering all that has ever happened to him and of perceiving everything that is happening everywhere in the universe. The function of the brain and nervous system is to protect us from being overwhelmed and confused by this mass of largely useless and irrelevant knowledge, by shutting out most of what we should otherwise perceive or remember at any moment, and leaving only that very small and special selection which is likely to be practically useful. An extension or modification of this type of theory seems to offer better hopes of a coherent synthesis of normal and paranormal cognition than is offered by attempts to tinker with the orthodox notion of events in the brain and nervous system *generating sense-data.*

I shall present Mr. Flew's contentions so far as possible in his own lively language. In his first talk, entitled "Minds and Mystifications," he said:

> Though parapsychologists have abandoned dramatics for the pedestrian round of laboratory routine, most of them still seem to compensate themselves with the conviction that their work has profound metaphysical significance. In these talks, I shall suggest that whatever may be the truth about these phenomena, this at least is a mistake: a mistake which derives partly from misunderstanding the logic of the terms in which they are popularly described; and partly from applying, often unconsciously, explanatory models which are in vital respects certainly inappropriate.
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> But allowing that the psi-phenomena, the ESP(and even the PK) effects occur, surely this has not the slightest tendency to prove Professor Rhine's dualist metaphysical conclusions? . . . No one has found, or even claimed to have found, elusive objects called minds, interacting with familiar things like cards and dice. All that has been found is that some people can guess cards right far more often than they have any business to do (and some perhaps can even influence dice by just "willing"). These are remarkable discoveries: but they prove no metaphysical conclusions. The source of the trouble is, I think, the "mind"-terminology in which Professor Rhine insists on describing and interpreting his results. He misconstrues its logic. . . . This is what Professor Rhine, and others, seem to have done. Taking the word "mind" to refer to some object, some sort of not-brain, he assumes that minds and brains can significantly be said to interact; and inter-

> prets his results in terms of this supposed interaction—which, not surprisingly, is found to be mysterious. In his own words, "Science cannot explain what the human mind really is and how it works with the brain." This is to make a mystery out of a muddle. "Mind" is not that sort of word at all. He complains that the student "finds in modern psychology very little on the mind as a distinct reality. Instead he studies 'behavior' and its relations to brain fields and pathways." But this does not convict psychologists of shirking the study of the mind: studying certain human capacities, feelings and performances is—is what is meant by—studying the mind. Mind talk is an alternative description of the same phenomena: it does not help to explain those phenomena; nor does it record the occurrence of further phenomena.

In his second talk, entitled "A New Name for Guesswork," there are two main themes, the first being to dispose of the view that precognition has metaphysical implications. Flew treats separately two philosophical issues which are raised on account of precognition: (a) puzzles about free-will, and (b) the "apparent implication that causation can work backwards in time." Regarding (a) Flew says : "ESP raises no new issues here which are not already raised by scientific prediction and common-sense forecasting," and he goes on to deny that there is "any necessary and general incompatibility between predictability and freedom in human conduct. . . . We can foresee many weddings which will not be shot-gun weddings." He concludes that "even if precognition were (what it is not) a species of knowing, it would still not have the slightest tendency to show that people never act of their own free-will." Flew then dismisses (b) by saying that the proposition that a cause must be prior to its effect is not a matter of fact but "an analytic proposition the truth of which depends solely and entirely upon the meaning of the terms 'cause' and 'effect.'" "No evidence," he concludes, "ever could imply that causation can work backwards in time."

Flew's second theme is to argue that some at least of our puzzlement about ESP is due to our use of the model of perception. The remedy he suggests is to think of it as guesswork. "It might be useful to think of ESP not as a kind of perception but as a species of guesswork. Then we should study ordinary guesswork to find why subjects guess one way and not another when no ESP factor

is involved: further knowledge of the normal might throw into relief further and characteristic features of the abnormal."

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I shall now discuss some of the wider issues that have been raised in these papers. I imagine that most readers of this JOURNAL will find the views of Mr. Flew most provocative, and I shall therefore discuss them first and at greatest length.

First, I want to ask whether there is anything of value in Flew's suggestion that "ESP" is "a new name for guesswork." I am afraid I cannot understand why Flew should think that this suggestion might be useful. Is his point that calling them "guesswork" is an appropriate way of *describing* the facts in question, or that so thinking of them will help to *explain* the facts? In either case the suggestion seems unacceptable. "Trying to guess" is an appropriate description of the state of mind of the subjects in most ESP experiments, and the public may sometimes need to be reminded of this; but Flew seems to forget that this is not an appropriate description of the state of the subjects in spontaneous cases, and a term which suggested that it was would be highly misleading. If Flew thinks that "guesswork" might be used as an explanatory concept, he seems to have been forgetting what has to be explained. We may grant that the guesses of the experimental subject are not random but are influenced by subjective associations which it might be of psychological interest to investigate. But how could this throw light on ESP? Dr. D. J. West has expressed the issue neatly in a recent criticism of Flew (**7**): "In a successful ESP test, there is a correlation between the cards and calls which shows that one of the associations available to the subject, and effective in determining his choices, is the target order. No amount of knowledge of the ordinary associations which influence choices seems likely to explain how the concealed target becomes available to the subject." Flew's proposal could be attacked from other directions, but I do not think more need be said to show that he has not provided a model which could usefully be substituted for that of perception.

Consider next Flew's claim that precognition has no relevance to the free-will problem, on the grounds that it raises no issues that are not already raised by normal prediction. Flew apparently thinks

that people who hold that precognition is relevant here are making a fairly simple mistake: the mistake of inferring, from the premise that some human actions have been precognized *or normally predicted,* the conclusion that in performing these actions the agent could not have been free to act otherwise. That such an inference is invalid can be readily agreed. If the predictability of a person's conduct were incompatible with his freely choosing such conduct, we should have to say that a man who acts consistently on a principle (for example, paying his debts) is not acting freely in so doing; but such a conclusion would plainly be unwarranted. The view that precognition is relevant to free will is not always, if it is ever, the result of making the above mistake.

Why then should precognition be thought to be relevant to the free-will problem in a way in which normal prediction is not? The answer as I see it is as follows. In order to explain precognitions it seems necessary to suppose that they are due to cognition of, or causal influence by, future events. But in order to enter into such relationships the future events would have to be in some sense actual before they happen. The success of normal predictions does not appear to have any such implications. This is because the making of normal predictions can be wholly explained in terms of events which preceded them (for example, observations, the formulating of laws, the unconscious formation of expectations, etc.). The fact that some future events are predictable does not seem to imply that any of them were actual before they happened, nor does it imply that all future events are predictable. The fact that some future events are precognized seems to imply that *these* future events were actual before they happened, and it is difficult to suppose that *some* future events could be actual while others were not. For in order to make sense of the notion of any future event being actual, it seems necessary to revise drastically our ways of thinking of time and change. The obvious way of doing this (perhaps not the only way) is to suppose that "Event E is happening" is equivalent, not to "E is coming into existence," but to "E is being disclosed to or made accessible to our minds"; that is, to suppose that the objects of our consciousness, future as well as past, are ordered timelessly in a fourth dimension and that our experience of things changing is

due to a transition of our consciousness along successive segments of this series of objects. This theory is, I think, incompatible with free will. It implies that we could never say of a person that he could have acted otherwise than he did, unless we mean (fatuously) that he could have acted differently *if* it had been a different world! And this incompatibility would have nothing to do with the question whether, or to what extent, normal prediction is possible. A world in which all objects of past and future experience are actual might be one in which no future experiences could be predicted, or might equally well be one in which all or most could be predicted. The extent to which prediction (that is, normal prediction) was possible would depend on the extent to which "patterns" were repeated in the objective order.

I am extremely reluctant to accept this theory of time apparently implied by precognition since, apart from theoretical difficulties, I feel convinced that I can play some part in shaping some future events, and am not merely a passive spectator as the theory implies. We might avoid having to accept this theory of time by denying that precognitive phenomena must involve cognition of, or causal influence by, later events. In fact Flew does deny this. However, his reasons for doing this are weak. His reason for rejecting the cognitive model is that the subject in ESP experiments cannot be said to know when his guesses are correct, if we use "know" in its everyday sense. No parapsychologist would disagree with this statement, but is it relevant? If cognitions are involved they are admittedly unconscious. Flew's reason for rejecting the causal model is that the current meanings of "cause" and "effect" make it tautologous to say "a cause must be prior to its effect." This may be granted, but to leave the matter there would be to ignore what constitutes the problem, that is, the fact that, in precognitive phenomena, the nature of the earlier event seems to be causally dependent on that of the later event, rather than the contrary. Whether this is really so cannot, I think, be settled by ascertaining the dictionary definition of "cause." It may be possible to explain away all apparently precognitive phenomena by exhibiting them as cases of delayed telepathic or PK influence, in which the later event is causally dependent on the earlier. I have explored this solution elsewhere (**5**). It does

not seem to me *very* plausible in view of: (1) spontaneous cases in which the events precognized are major catastrophes. I find it very hard to believe, for example, that someone's dream could cause a train crash or an earthquake. (2) Carington's famous experiments with drawings, in which the "hits" of a large number of subjects showed a temporal clustering. The facts strongly suggest that the subjects' responses were the dependent variables and Carington's selection of targets the independent variable.

I turn now to the thesis of Flew's first talk. Flew is here giving a forceful expression of the viewpoint of philosophical behaviorism, which was presented in Professor Gilbert Ryle's recent book, *The Concept of Mind.* This work is a sustained attack on Cartesian dualism, dubbed by Ryle "the myth of the ghost in the machine." Ryle did not attempt to give a behavioristic analysis of the concepts employed in parapsychology. In attempting to make good this omission, Flew attacks at the point where behaviorism might well be thought to stand in most need of defense.

First I want to question Flew's opinion that the occurrence of psi phenomena "has not the slightest tendency to prove Professor Rhine's dualistic metaphysical conclusions." Now I doubt if Rhine has ever claimed that psi phenomena *prove* dualism to be true, and I am sure that contemporary philosophers would not expect laboratory experiments to *prove* any metaphysical theory. But Rhine is in good company in his belief that psi phenomena are favorably relevant to psycho-physical dualism, and this belief Flew apparently wishes to discredit. The position as I see it is this. The main rival of psycho-physical dualism is physicalism, according to which all so-called mental performances and powers are merely functions of physical organisms. Flew, like Ryle, seems to take this theory for granted. The popularity of this theory derives mainly from the progress made by the biological sciences, notably physiology, on the strength of which it seems plausible to claim that all of our mental powers can be explained *in principle* by reference to physical mechanisms in the central nervous system. The sort of mechanisms which are invoked change as research proceeds. (Not much importance is now attached to the "conditioned reflex," but cybernetics has provided some much more promising concepts.) Now it does not cut

much ice to point out how very far the biologists are at present from their goal of reducing the mental *in detail* to the physical. They can point to certain established facts—for example, the dependence of specific mental powers on the integrity of specific regions of the brain—and they can describe the general lines on which their reduction is to be carried out. Even if this description is very vague—for example, that memory is to be reduced to "brain-traces"—a dispute would be inconclusive. The relevance of psi phenomena is, of course, that it seems impossible in principle to explain these powers in terms of physical mechanisms, or rather that, although at the first glance this seems possible in terms of a radiation theory, this type of explanation breaks down as soon as we try to apply it in any detail. In view of this I think we must conclude that the occurrence of psi phenomena weakens the case for physicalism and *pro tanto* strengthens the case for rival metaphysical theories.

A second point on which I want to challenge Flew is this: After referring to the fact that what psychologists study is human performances and their relations to brain fields, he says, "Mind talk is an alternative description of the same phenomena: it does not help us to explain those phenomena." I should not deny that this may be true of much so-called "mind-talk"; for example, "He has a clever (quick, adaptable) mind." But is Flew right about all "mind-talk," about all use of mental concepts? This suggestion can, I think, be countered by a single example. The concepts used by Freud in formulating his revolutionary theories were certainly not merely describing in different words what was already known or conjectured about human performances or cerebral processes; and Freud's "mind-talk," even if sometimes extravagant, has certainly helped to explain a wide range of facts—not only features of normal experience, like dreams and errors, but many problems in abnormal psychology with which no progress was made by the psychiatrists who employed the mechanistic concepts provided by biology.

Flew might reply by claiming: (1) that the physiologists may some day be able to correlate Freud's "repressed desires," etc. with specific brain processes; and/or (2) that Freud's theory can be interpreted, in retrospect, as a shorthand way of describing and correlating a large number of facts about the dispositions acquired by

human organisms as the result of their social intercourse and crises. However, even if for the sake of the argument we granted these uncertain claims, they would not dispose of the crucial point that Freud would not have made his important discoveries if he had not used mental concepts, if he had not, for example, conceived desires as entities which have a mode of existence and operation different in kind from the entities postulated by the other sciences. Flew seems to be urging parapsychologists to eschew mental concepts in their attempts to explain their findings. It seems to me to be a sufficient answer to point out how much we should have lost if Freud had followed such advice.

Flew is guilty, to put it mildly, of overstating his case. Up to a point I sympathize with his protests. I think he is right to stress that so long as other scientists feel that the main aim of parapsychologists is to vindicate metaphysical dualism, they will tend to be suspicious and to ignore or discount its discoveries. Again, I should concede that at some levels of discourse "mind-talk" may be misleading, or at any rate question-begging. The parapsychologists' data are obtained by methods to which the behaviorists cannot object. So long as no one can provide an adequate explanation of all the data, it is very desirable that all parties should be able to refer to and describe the facts in the same terminology. For describing and classifying the data we ought therefore to use a terminology, like the "psi" terminology, which does not presuppose or suggest any metaphysical theory. This will focus arguments on the most important problem, that of *explaining* the data. It is to be hoped that many more neo-behaviorists will try their hand at this. Their approach may not yet have been pressed hard enough; but until or unless they succeed, they are not entitled to criticize others for trying different approaches. In trying to develop the lines of thought suggested by Price and Broad, one's conscience need not be troubled by anything Flew has said!

Price offers us an ingenious and original theory which enables us to regard telepathy as a species of PK, or possibly vice versa. This is a notable achievement, since "mental" and "physical" psi phenomena had for so long appeared to constitute an irreducible dichotomy. Some criticisms can be made of Price's language. For

example, when he recommends that we take the *idea* as our psychological unit, he says he will assume that the meaning of this term is sufficiently well understood. This suggests that he is using "idea" in an everyday sense. In fact, he uses "idea" to refer to a hypothetical entity, which exists in a "common unconscious" and which produces what we normally call ideas (for example, mental images). There is no objection to postulating such entities, which would have the same logical status as the physicists' electrons, etc., but it would be better to give them a different name. It may, however, be unfair to press criticisms of this kind, since Price's papers were not addressed to philosophers or scientists.

The most obvious short-coming of Price's theory is that it takes no account of clairvoyance. Price makes no attempt to discuss how ideas are acquired, and clairvoyance, he says, is a way in which ideas are acquired. (We usually think of telepathy also as being a way in which ideas are acquired. This is because we think of telepathy from the viewpoint of the "percipient," whereas Price is thinking of it rather from the viewpoint of the agent.) Price's theory is thus incomplete. Even if it were still possible to explain away the experimental evidence for clairvoyance in terms of precognitive telepathy, I doubt if Price's theory would be adequate, since on this theory it would be particularly difficult to explain precognition. Price would have to account for cases where a person A has a precognition at time 1 of a particular event which B perceives by means of his senses at time 2. How could B's idea (percept) be the source of A's earlier experience, if B's idea did not originate (come into existence) until time 2? On Price's theory the occurrence of precognition would seem to imply the actuality not only of future *physical* events, but also of future *mental* events, for example, "acts" of sensing or perceiving!

Now it is interesting to notice that the theory which Broad recommends seems to be answering the very question that Price left aside concerning the way in which ideas are acquired. The two theories appear to be mutually complementary. Price's theory offers us a unified account of PK and telepathy as manifestations of an unfamiliar way in which ideas *operate;* and, Price also suggests, we may regard the normal way in which ideas affect matter (that is,

via efferent nerves and muscles) as being a particular manifestation of their unfamiliar *modus operandi*. The Bergsonian theory provides a unified account of clairvoyance and normal sense perception as being (to use Price's language) different ways in which ideas are *acquired*. Each theory covers the psi phenomena to which the other does not seem applicable; for the Bergsonian theory would not be relevant to PK, and, prima facie, it fits clairvoyant rather than telepathic ESP. My noticing that these two theories do complementary jobs naturally prompted me to hope that we might by combining them provide a theoretical framework adequate for all of the facts. I am afraid, however, that this program is not an easy one to implement, and I shall conclude by offering some reflections on it.

1. Although they do complementary jobs, the two theories do not, as they stand, fit together at all neatly. (a) It seems inappropriate to class clairvoyance along with normal sense perception as "a way in which ideas are acquired." This would suggest that people can acquire new concepts by clairvoyance, instead of by abstraction from the data provided by the senses or introspection. I know of no evidence for this. Clairvoyance and telepathy seem to be alike in this—that when paranormal information is received in the form of conscious ideas (concepts or images), the ideas in question have already been acquired by the subject by normal means. We could, however, group clairvoyance and sense perception together as different ways of "acquiring information," or of "responding to the environment."

(b) A more serious matter is this. Each theory postulates an inhibiting mechanism to perform an indispensable function. In Bergson's theory the function is to block a mass of irrelevant information about remote events; in Price's, to prevent the irruption of a mass of irrelevant ideas from the "common unconscious." In Bergson's theory the blocking is performed by a physical mechanism in the sense organs and/or the central nervous system; in Price's, by a mental barrier (the model presumably is Freud's "censor").

To combine the two theories by mere addition would be clumsy to say the least. I suggest that if we are to adopt the Bergsonian theory at all, we might as well try to apply it to telepathy as well as to clairvoyance. For if we are to postulate a mysterious direct

awareness of an indefinite range of events, why suppose that this awareness is limited to *external* features of things, such as sight and touch reveal, as distinct from mental processes in other people which are normally revealed only to their own introspection? In that case, however, the concepts of a common unconscious and of a second (mental) inhibiting mechanism would be superfluous. Something would still remain of Price's theory—his way of assimilating PK and telepathy as manifestations of the causal efficacy of a person's ideas. But Price's theory and Bergson's seem to involve different ways of thinking of telepathy. When, on Bergson's theory, we should say that P is directly aware of A's experience, we should, on Price's theory, say that an idea in A's consciousness "gets into" (or directly produces effects in) P's consciousness. However, the difference here may be merely terminological. If we formulate Bergson's theory in terms of a direct *responsiveness to* (instead of *awareness of*) an indefinite range of events, the statements that P responds directly to A's experience, and that A's experience directly produces effects in P's mind, could be treated as equivalent.

2. As it stands, the Bergsonian theory would cover clairvoyance, including the retrospective variety, and I have suggested that we may extend it to cover telepathy. Can we also extend it to cover precognition? I think it may be of interest to mention something Broad said in 1937 in the course of a symposium on precognition (1). Price had suggested that we try to explain ESP by a theory like that of Bergson. Broad asked if this would mean that "were it not for special inhibiting factors, each of us would automatically know at every moment of his life all facts about the whole of the past, the present and the future? Is this suggestion even intelligible, let alone credible?" Interpreted thus, the theory is that each of us is potentially *omniscient;* and to explain men's rare and sporadic exercise of ESP by postulating universal omniscience does seem extravagant. We might try to mitigate this extravagance by postulating limits to the range of our potential awareness (responsiveness). If we do this we have on our hands a theoretical problem which does not arise if we postulate omniscience ("omni-responsiveness"); that is, to explain why, since we are abandoning the orthodox view as to the limits of our potential awareness, we should

postulate *any* limits. This theoretical problem would not be solved by discovering empirically what are in fact the limits (spatial, temporal, etc.) of the range of ESP. Yet on the other hand, until we have much more extensive evidence as to what in fact these limits are, it seems premature to postulate definite limits and try to provide a theoretical explanation of these. The main difficulty with the Bergsonian theory is to extend it to cover precognition. To attribute precognition to awareness of, or responsiveness to, future events seems to commit one to the paradoxical theory of time which I described earlier. I conclude, then, that the task of extending Bergson's theory and working it out in detail is a very formidable one, but I hope the difficulties will challenge, and not deter, further efforts.

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A CONTINUATION OF THE EXPERIMENTS IN PLACEMENT PK

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ABSTRACT: This article reports further work by Mr. Forwald in which he released cubes of various materials from a machine, trying consciously to "influence" them so that they would fall on a specified area of a dice table. Three cubes each, of two different materials, were used in each series, but the subject tried to influence only one of the two kinds. In the three new series (Section 3 of the experiments) reported here, the cubes were steel and aluminum, rough bakelite and smooth bakelite, wood and paper. Steel, rough bakelite, and wood were the ones the subject tried to influence. They gave total scores which were highly significant. The not-influenced cubes were at the chance level, and there was a statistically significant difference between the two groups. The three control series in which there was no attempt to influence either kind of dice consistently gave chance results also. When the data of this report are combined with those of the two sections reported previously, the overall results of throws on which the subject attempted to influence the dice by PK show an accumulated deviation of +691 in 71,500 die-throws, which has a CR of 5.17 and a probability of 3 in 10,000,000.

Section 3 strengthens the evidence of two secondary effects found in the earlier results: position effects within the five successive throws for the same target and a decline between the two halves of the twenty-throw double set.—Ed.

THE PRESENT WORK is a continuation of that described in an introductory report by J. B. Rhine (2) and in a later report by the author (1). The former dealt with Section 1 (Series 1 and 2 and Control Series A and B) and the latter with Section 2 (Series 3-11) of my PK placement experiments. The general objective of the tests was to influence cubes, released from a machine, to fall on a specified portion of a dice table. The present report describes the results of Section 3, comprising three placement series, 12, 13, and 14, and three control series, C, D, and E. As in the previous tests, I myself was the only subject.

A consistent feature in Section 3 is that all throws involve three cubes of two different kinds, totaling six cubes per throw. In the three placement series a novelty of a psychological kind was introduced, it being fixed in advance of each series that only a specified one out of the two kinds of cubes was to be "influenced" to hit the target area; whereas no conscious consideration was to be given to

what would be the results of the other kind of cube. (The dice selected to be influenced in each series were the ones that would appear to be physically more difficult to control, either the heavier ones, when there was a difference in material, or the rougher ones when all six were of the same material.) In the previous placement experiments the subject consciously attempted to get both kinds of cubes into the target area if two kinds were released at a time.

Procedure

The apparatus was the same as in the previous experiments. It had an electrically controlled channel, the front end of which dropped and released the dice when I pushed a button at the end of a cord. The row of dice then rolled down an inclined plane and scattered over the horizontal surface, which was divided longitudinally into two equal areas, designated as A and B. In Placement Series 12 and 13 and in Control Series C and D the division was made by a cord stretched across the surface down the length of the table. Its thickness was sufficient to give a noticeable incline to any cube resting upon it, thus making it possible to determine the area which the cube favored.

In Placement Series 14 and in Control Series E the cord was replaced by two ink lines, one on each side of and parallel to a dividing middle line, and at a distance of 15 mm. from it. The distance between the ink lines thus was 30 mm. The cubes had a side of 16 mm. and a side diagonal of 22.7. If a cube located in the alley between the side lines just touched one of them, a gap of at least 7.3 mm. would thus exist between the cube and the other line. Cubes falling outside the sidelines and on them, as well as those in the alley which touched the line on the target side, were counted as hits. Cubes coming to rest definitely between the sidelines of the alley were not counted as hits, even though their middle point was clearly on the target side of the middle line. It is obvious that this method of recording is on the safe side; that is, it gives a lower number of hits than the method with the cord, in which the tilt of the dice falling on the center line provided an objective basis for judgment. The double ink line arrangement was provided to bear upon a question which is not dealt with in this report, and further consideration on this point will not be necessary here.

All the series in Section 3 were of the same pattern as those in Section 2; that is, sets beginning with five throws on the A-side as target, followed by five throws on B, were alternated with sets beginning with five throws on B, followed by five throws on A; and the total number of throws was 3,000 for each series. Chronologically, the six new series covered in this report were carried out in the order 12, 13, 14, C, D, E.

In Placement Series 12 three steel cubes and three aluminum cubes were used. Conscious attempts were made to get the steel cubes to settle in the target area. In Series 13 all six cubes were of bakelite, three with rough surfaces and three with smooth surfaces. Attempts were made to influence only the rough cubes. In Series 14 the influenced cubes were of wood and the cubes to which no attention was paid were hollow paper cubes. In Control Series C, D, and E the cubes were the same as in Placement Series 12, 13, and 14, respectively.

No conscious effort was made to influence any of the cubes in the control series. They were released immediately after being placed in the container, whereas in the placement series some seconds of "concentration" on the kind of dice selected to be influenced preceded the release.

Results

General Results

The results of the placement series are given in Table 1 (the last three columns of which contain data that will be discussed later under "Order Effect"). A distinction is made there between the two kinds of cubes; that is, the kind which was specified to hit the target, and the kind to which no attention was paid. The first kind, or the influenced cubes, gave positive deviations for all three series, but a significant deviation only for Series 14. The total deviation for the 4,500 falls of the combined three series for influenced cubes, is +134, with a CR of 4.0 (P = .000,07). The second kind of dice, the not-influenced, gave deviations at the chance level, the total deviation for the 4,500 throws being −10, with a CR of .3 (P = .76). The CR_d for the influenced and not-influenced dice is 3.04 (P = .0024).

Table 1

GENERAL RESULTS FOR PLACEMENT TESTS OF SECTION 3

Series	Type of Dice	Number of Die-Throws	Pooled Dev. A-and B-side	CR	P	AB-BA Diff.	CR_d (AB-BA)	P
INFLUENCED DICE								
12.....	Steel	1500	+23	1.19	.24	−15		
13.....	Rough Bakelite	1500	+39	2.01	.044	+15		
14.....	Wood	1500	+72	3.72	.000,2	+44	2.27	.023
Total.....		4500	+134	4.0	.009,07	+44		
NOT-INFLUENCED DICE								
12.....	Aluminum	1500	+13	.67		+31		
13.....	Smooth Bakelite	1500	−11	.57		+47	2.43	.015
14.....	Paper	1500	−12	.62		+10		
Total.....		4500	−10	.30	.76	+88	2.62	.0088

CR_d (Influenced-Not Influenced) = 3.04
P = .0024

The three control series (C, D, and E) gave results that are at the chance level: 9,000 die-throws with a deviation of −8.

Table 2 gives a summary of the placement results for Sections 1 and 2 reported previously (**1, 2**) as well as those for Section 3.

Only the results of the influenced dice for Section 3 are considered. The placement experiments in Sections 1 and 2 regularly

Table 2

SUMMARY OF PLACEMENT RESULTS OF SECTIONS 1, 2, AND 3

Section	Series	Number of Die-Throws	Dev.	CR	P
1.....	1, 2	40,000	+500	5.00	.000,000,6
2.....	3 to 11	27,000	+ 57	.69	.49
3*.....	12 to 14	4,500	+134	4.00	.000,07
Total.....	1 to 14	71,500	+691	5.17	.000,000,3

*Results of influenced dice only

involved only influenced dice. Hence it is justifiable to include for comparison with them only the results of the influenced dice in Section 3.

It is seen in Table 2 that the total of 71,500 throws in which conscious attempts were made to get the dice to settle in the target area gave 691 hits above the mean chance expectation of 35,750 hits. This result has a CR of 5.17, which is a highly significant value (P = .000,000,3).

Position Effects

U-Curve. It was shown in the previous report (1) that the deviations in the five positions constituting the half-set (that is, the five successive throws with one area as target) represent a marked U-curve which cannot be attributed to chance. Although the deviations in Section 3 do not show the U-curve, the overall U-curve effect nevertheless is intensified when the results are taken in combination with those of Sections 1 and 2. This is mainly due to the high deviation in the first position in Section 3. The intensification is more pronounced if the deviations of both influenced and not-influenced cubes in the placement series of Section 3 are considered than when the influenced dice alone are taken into account. The position effects of the not-influenced dice show a U-curve ($\chi^2 = 3.43$, 1 d.f.; P = .06) which suggests that they may have been affected to some degree by PK in spite of my intention to ignore them.[1] The position results for the five-throw unit are given in Table 3 in terms of how well the data fit a U-curve. In this table the first two sections, previously reported, are summarized; Section 3 is presented in detail; and the evaluation of the placement results of the three sections combined is shown. It can be seen that the U-shape is not in evidence in the results of the control series.

The foregoing analysis of position effects shows that the scores in the five throwing positions give a significantly close approximation to a parabolic or U-curve. If there had been a clear-cut precedent for predicting that this particular distribution of hits would occur, this finding would provide an adequate basis for concluding

[1] The statistical method for applying a chi-square test to see how closely the data approximate a quadratic, or U-, curve is given in the footnote on p. 64 of my previous report. The closer the fit to a U-curve, the higher is the value of χ^2.

Table 3

POSITION EFFECTS FOR SECTIONS 1, 2, AND 3

Section	Series	Throw Number 1	2	3	4	5	χ^2 (Quadratic Curve)	P
PLACEMENT SERIES								
1	1, 2	+159	+45	+48	+75	+173	7.17	.007
2	3 to 11	+ 70	+25	−56	− 2	+ 20	3.83*	.05
Total		+229	+70	− 8	+73	+193	10.96	.0009
3a (Influenced dice)	12	+ 15	+ 3	− 1	+ 6	0		
	13	+ 18	− 7	+11	+10	+ 7		
	14	+ 31	+15	+13	+ 6	+ 7		
Total		+ 64	+11	+23	+22	+ 14	1.88	.17
3b (Not-influenced dice)	12	+ 7	− 5	+ 6	− 4	+ 9		
	13	+ 1	−13	0	−10	+ 11		
	14	+ 2	+ 2	− 7	−10	+ 1		
Total		+ 10	−16	− 1	−24	+ 21	3.43	.06
3a+3b		+ 74	− 5	+22	− 2	+ 35	5.20	.022
1+2+3a		+293	+81	+15	+95	+207	12.60	.0004
1+2+3a+3b		+303	+65	+14	+71	+228	15.16	.0001
CONTROL SERIES								
A		− 14	+ 3	− 5	− 3	+ 6		
B		− 41	+ 3	+11	−46	− 5		
C		+ 1	− 5	+13	0	+ 12		
D		− 4	− 6	0	+ 3	+ 1		
E		+ 2	−19	− 1	− 5	0		
Total		− 56	−24	+18	−51	+ 14	.145	.70

*This figure was incorrectly given as 2.58 in the previous report

that there are significant position effects in the data. There may be some question, however, whether this is the only form of curve that might have been expected on the basis of previous position effects. In fact, it must be admitted that the decision to fit the data to a quadratic curve was made after the data had been tabulated and when it was evident on the basis of inspection that the distribution followed a U-curve.

A more appropriate beginning point for evaluating the position

effects is to ask whether there is any *general* variation in the hits of the five positions beyond what may reasonably be attributed to chance fluctuation. If position itself is not a factor in PK placement, the expectation is that the total hits should be equally divided over the five throws for a given target. Any significant departure from such an even or rectangular distribution of hits, regardless of the shape of the curve, would offer direct and clear-cut evidence of position effects. A chi-square test was applied to the two-by-five contingency table of hits and misses for the five throwing positions of Sections 1 and 2 and the influenced dice of Section 3 combined. The value of chi square was found to be 13.73 (4 d.f.), with $P < .01$. (A slightly higher value of chi square is obtained if the not-influenced dice of Section 3 are also included, so it cannot be said that I made a selection of data in order to favor statistical significance.) This analysis shows that the position effects are statistically significant without regard to any particular hypothesis concerning the form of the distribution. We may therefore consider the former analysis in terms of the U-curve as one that is based upon a genuine parapsychological effect and that, at the same time, carries us one step forward toward an understanding of the position effects.

Order Effect. In the previous report (1) another type of position effect in the results of Sections 1 and 2 was described. This effect, designated as an "order effect," is characterized by a significantly higher scoring rate in sets started with the A-side as target than in those started with the B-side as target. (As previously mentioned, one set comprises five throws with one area as target, followed by five throws with the opposite area. Sets with the target order AB for the 10 throws alternated with sets using the target order BA.) The results of Section 3 were likewise examined with respect to this order effect, or AB-BA decline.

The differences in deviation between AB and BA sets for the placement results of Section 3 are given in Table 1. Those differences which represent a decline in scoring from AB sets to BA sets are marked with a plus sign, while the differences representing an AB-BA incline are given a minus sign. It is seen in Table 1 that the total AB-BA difference with the influenced dice is +44, and with the not-influenced dice, +88. This latter difference corresponds to

a CR of 2.62 (P = .0088), so that we again have strongly suggestive evidence that PK was operating on the not-influenced dice even though there was nothing significant in the total deviation. The difference for both influenced and not-influenced dice taken together is +132 (CR_d = 2.78, P = .0054).

The AB-BA difference in the control series was at the chance level.

Discussion

Section 3 represents a recovery of a significant overall rate of scoring in the PK tests as contrasted with Section 2, which was not significant in terms of its total deviation. The better results appear to me to be due to more favorable psychological conditions which I would list as follows:

1. The specifying in advance of each series that only one kind of cube was to be influenced increased my interest in the experiment, the more so as the difference in deviation between influenced and not-influenced cubes was observed to become larger from series to series. It is surprising, however, that the positive scoring with the influenced dice continued after it was observed in the first series. One might have expected that it would disappear as soon as I became aware of it, as was previously experienced with the order effect observed in Sections 1 and 2. There is, however, a difference in the two situations. In Section 3 a large deviation with the specified kind of cube was consciously aimed at, whereas the order effect in Sections 1 and 2 was only a secondary effect, one apparently caused by unconscious efforts.

2. The method of "concentration" in Section 3 was a different one from that used in Section 2. Instead of making a strong-willed effort to get the cubes into the target area, I behaved in a more calm manner, trying to get a mind picture of the favored dice as resting or moving within the target area. In many cases a strong feeling of "contact" with the cubes came on after some seconds, and when I pressed the button just after having obtained this feeling the throw almost always gave a positive deviation.

3. In the most successful of the series, Series 14, a further feature was added to the experiment. Only two sets were carried out on the same occasion, one set with target order AB, and one with order BA.

4. In all three series of Section 3 the results were summarized in each instance only after completion of the entire series. This gave a calm feeling throughout the experiment. In Section 2, the cumulative deviations were recorded successively at the end of each set as the series was going on, and I was anxious about the results toward the end of the 500 releases of each series. In several cases this anxiety apparently had the effect of producing negative deviations.

Series 14, with three wooden dice and three paper dice, brings up some interesting considerations. Conscious attempts were made to get the wooden dice to settle in the target area, whereas no attention was given to the paper dice. The results strongly indicate that the wooden cubes were influenced by some means to fall on the target side, but that the paper dice, thrown at the same time, were not influenced at all. The fact that this occurred although the wooden cubes are about six times as heavy as the paper cubes indicates that the means of influencing the objects are of a differential kind that may work in a selective way on the cubes. If the forces are considered as a sort of field effect working laterally in the direction of the target side, one would expect that the paper cubes would primarily have been influenced, as they are by far the lighter ones.

As may be seen in the previous report (1) on Section 2, there are signs that a differential mechanism of this kind may have operated also in Series 8, with forces working in opposite directions on wooden cubes and aluminum cubes.

The difference in paper and wooden cubes found in Section 3 is not due to the fact that paper cubes cannot be influenced by PK. The last part of Series 5 in Section 2 (with three wooden dice and three paper dice thrown simultaneously in attempting to influence both kinds) gave results which strongly suggest that the forces are able to work on hollow paper cubes also. The last 16 sets of Series 5 were carried out in succession at one session on September 27, 1950, and the results are the following:

	Paper Dice			Wooden Dice			
	Die-throws	Dev.	CR	Die-throws	Dev.	CR	CRd
First 8 sets.....	240	+32	4.13	240	— 5		3.38
Last 8 sets.....	240	+ 4		240	—18	2.32	2.01
Total 16 sets....	480	+36	3.29	480	—23	2.10	3.81

These results are strongly indicative of a real effect working in opposite directions on paper and wood. It appears, then, from Series 5 and 14, that both the paper and the wooden cubes may be susceptible to the forces involved.

It is interesting to note that the order effect (AB-BA decline) has again repeated itself in Section 3 after its sudden disappearance upon the discovery of the effect in Section 2. The order effect was not present in any of Series 5 to 11. No definite explanation can be given for the fact that the AB-BA decline reappeared in Section 3 after it was absent in Section 2, but the following considerations may be relevant.

This effect was not discovered until all the series of Section 1 had been completed, when it came to light in the analysis of the data. During Series 3 to 11 (Section 2) I was constantly on the lookout for the same trend and kept a running total of the AB-BA difference after each such double set of trials. By the time I started Series 12, I had given up any hope of finding further evidence of this decline and therefore I no longer checked for it until the end of each series.

In Series 12 and 13 the effect did not occur with the influenced dice in which I was particularly interested but was present in the results of the not-influenced dice. Only at the end of Series 14 did I discover that the influenced dice, which had given a significant total deviation on the target, also showed a significant difference between the AB and BA sets. These facts suggest: (1) that this decline effect occurs in my PK placement results in series which yield a significant deviation, but only when I am not constantly on the alert or anxious about this particular result; (2) that it may occur in throws which do not yield a significant total deviation if I am not consciously trying to influence the dice (as with the not-influenced cubes of Series 12 and 13).

As previously mentioned, the Control Series C, D, and E were conducted after the conclusion of Placement Series 12, 13, and 14. There seems to be a carrying-over of the effects of the placement series into these control series. That is, in Series C, the first of the control series to follow after the placement series, there is a relatively high positive deviation with the influenced kind of dice and relatively high AB-BA declines with both kinds of dice, though

there was no conscious effort to affect these dice in any way. In Control Series D and E, the corresponding deviations are in no way outstanding; the effect appears to have died out. The repeated conscious efforts probably had built up a psychological state which was kept up until after completion of Control Series C. The discovery of this trend in the results after completion of Series C may have caused a change in my state of mind, this being the reason why the effect cannot be traced in the remaining control series. It may be mentioned that Series 14 was terminated on March 16, 1952, and Series C was not started until May 8. The special, unconscious state of mind, if it did exist, would thus have been held over for about two months' time.

References

1. Forwald, H. A further study of the PK placement effect. *J. Parapsychol.*, 1952, **16,** 59-67.
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A SECOND SURVEY OF PSYCHOLOGICAL OPINION ON ESP

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ABSTRACT: This is a repetition of a survey sent out by Dr. Warner and Dr. C. C. Clark in 1938 to determine the general attitude of psychologists toward the research in parapsychology. In this new survey, questionnaires were sent to 515 Fellows of the American Psychological Association. Seventy percent replied. About 89 percent of these indicated that they consider the investigation of ESP a legitimate scientific undertaking; 78 percent consider it to be within the province of academic psychology; and 36 percent express in varying degrees their belief that the experimental approach, as exemplified by research reports in the Journal of Parapsychology, is in the proper direction. About three-quarters of the respondents have the view that ESP is either a "remote possibility" or "merely an unknown." A comparison of the pattern of replies for the two surveys shows little variation in the intervening fourteen years.

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IN THE SUMMER of 1938 a survey was made by mail of the 603 Members of the American Psychological Association (2). This survey was in the form of a questionnaire and it was designed to find out the general attitude toward the research in parapsychology on the part of the psychologists polled.

This summer, fourteen years later, the survey has been repeated, using a sample of the now more than fifteen hundred Fellows of the Association, the status of Fellow being approximately tantamount to the status of Member at the earlier date. The sample was a random one totaling 515 names and it was drawn by selecting every third one of the Fellows as they are listed in alphabetical sequence in the 1951 Directory. Seventy percent (360) replied, using the prepaid, addressed envelopes provided.[1] Fourteen years ago only sixty percent (361) replied. This larger return almost exactly offsets the fact that a slightly smaller group was questionnaired. Eleven of those responding disqualified themselves for one reason or another, chiefly on a "No Opinion" basis.

[1] Late returns raise this figure to 74 percent but do not significantly alter the results here reported.

The questionnaire, a one-page mimeographed sheet, is included in Table 1. It duplicated exactly the one used in the earlier survey, with this exception: In Question II the word *books* was used rather than the word *book* because of Dr. Rhine's publications since 1939. It was realized that on the basis of our experience with the earlier survey the questionnaire could have been improved in some respects, yet it was my feeling that it was more important to use the earlier form so that the results could be directly compared with those of the earlier study.

The raw data of the second survey are presented in Table 1 in a form comparable to that used in the article reporting the 1938 results. The italicized portions of this table represent the actual wording of the questionnaire. The unitalicized phrases did not appear in the questionnaire. They represent categories under which volunteered comments were classified. As in the previous survey, the margins and back of many of the questionnaires were covered with interesting and often lengthy comments. We wish to express our appreciation of the patience and courtesy of those who went to such trouble to make their replies as accurate and complete as possible.

Placing volunteered comments in categories entailed some subjective interpretation on my part, but every effort was made to avoid doing violence to the opinions expressed. The categories used in the first survey for volunteered comments were in general suitable for the current study. Thus, for the most part, the original categories have been retained so that direct comparison can be made by referring to the earlier article. One exception concerns the question of which scientific field parapsychological research should come under (Part *c* under Question III, "Where else?"). It was necessary to add three more categories in this group: "Clinical psychology, psychiatry," "Experimental psychology or biology," "Science fiction." Another exception involves Part *b* under Question IV concerning criticisms or experimental methods which the respondent might suggest. It seemed best to set up an entirely new group of categories for the answers to this question since many of the earlier criticisms have been answered and the suggestions pertaining to them have therefore become outmoded. Table 2 presents a comparison in per-

Table 1

RESULTS OF 1952 QUESTIONNAIRE

I.	*In your opinion, is "extrasensory perception"*						
	1. *An established fact?*	119	36	136	49	9	
	2. *A likely possibility?*						
	3. *A remote possibility?*						
	4. *An impossibility?*						
	5. *Merely an unknown?*						
II.	*Is this opinion based on*						*Total*
	1. *Reports in newspapers and magazines?*	28	6	35	11	3	83
	2. *J. B. Rhine's books?*	56	15	67	24	7	169
	3. *Experimentation as reported chiefly in the Journal of Parapsychology?*	36	7	55	18	6	122
	4. *Hearsay?*	11	2	13	1	1	28
	5. *Purely a priori grounds?*	44	18	37	11	1	111
	6. Other						
	My scientific background	5	3	6	0	0	14
	Professional meetings	2	0	2	0	0	4
	Discussion with colleagues	10	1	6	6	1	24
	Reports in sources other than *Journal of Parapsychology*	17	8	21	2	2	50
	Discussion by Rhine	2	2	2	1	0	7
	Personal experience	10	2	12	5	0	29
	Personal experimentation	4	5	3	2	0	14
III.	a. *Do you consider the investigation of ESP a legitimate scientific undertaking?* Yes	105	19	128	49	9	310
	No	9	16	7	0	0	32
	Blank	5	1	1	0	0	7
	b. *If so, does it fall within the province of academic psychology?* Yes	86	19	121	38	8	272
	Partly	5	1	1	3	0	10
	No	16	8	8	5	1	38
	Blank	12	8	6	3	0	29
	c. *Where else?*						
	Nowhere else	1	1	0	1	0	3
	Nowhere at all	0	0	0	0	0	0
	In an entirely new field	4	0	0	0	0	4
	Physiology, neurology, etc.	3	2	4	0	1	10
	Abnormal, applied psychology	0	0	2	2	0	4
	Sociology	1	0	0	0	0	1
	Physics	3	2	1	1	2	9
	Mathematics, statistics	2	0	0	0	0	2
	Philosophy	1	0	1	0	0	2
	Theology, religion, mysticism	2	0	1	2	0	5
	In any laboratory	2	0	3	0	0	5
	In any *private* laboratory	1	0	0	1	0	2
	By committee of scientists	0	0	2	0	0	2
	By societies of psychical research	1	0	1	1	0	3
	Supported by a foundation	0	0	0	0	0	0
	With cooperation of magicians	1	0	0	0	0	1
	With cooperation of detectives	0	0	0	0	0	0
	Clinical psychology, psychiatry	3	0	3	4	1	11
	Experimental psychology or biology	2	2	2	1	1	8
	Science fiction	1	0	0	0	0	1

IV. a. *In your opinion is the present experimental movement (as exemplified by the better controlled work reported in the Journal of Parapsychology) in the proper direction?* .... Yes	34	3	40	23	7	107
Probably	2	1	2	1	0	6
Partially	5	2	3	5	0	15
No	18	17	23	4	0	62
Probably not	7	0	4	2	0	13
Blank and doubtful	14	9	24	1	1	49
Have not read it	39	4	40	13	1	97
b. *If not, what criticisms have you, or what methods to suggest?*						
Personality relationships	1	0	0	3	0	4
Experimental design	17	5	26	5	0	53
Eliminate bias on part of investigators	10	9	14	0	0	33
Investigate reported phenomena	0	1	1	1	0	3
Fit present ESP knowledge into theoretical frame	1	1	0	2	0	4
Lack of philosophy; neglect of spiritual point of view	1	0	2	0	0	3
Individual differences in ESP subjects	0	0	5	0	0	5
More important things to study first	3	2	5	0	0	10
Quit altogether	1	6	0	0	0	7
Testable hypothesis cannot be formulated	0	1	0	0	0	1
Study minimal cues, perception	1	1	0	0	0	2

Filled-in questionnaires returned	349
Failed to return questionnaires	155
Refused for various reasons to fill in questionnaires	11
Total	515

centages of the results of answers to the italicized questions for the two surveys.

Results

In this brief discussion of results we will follow the questions in the order asked.

Question I

Here the pattern of replies in the two surveys is remarkably similar in that almost three-quarters of the respondents in both surveys selected either "remote possibility" or "merely an unknown" as the phrase best describing their position (see Table 2). The slight shift which occurred, however, is interesting. The respondents stating that they feel ESP is either a "likely possibility" or actually "an established fact" have risen from 8.8 percent to 16.6 percent. Those flatly calling ESP "an impossibility" dropped slightly from 14.5 percent to 10.3 percent.

Table 2

COMPARISON OF RESULTS OF 1938 AND 1952 SURVEYS BASED ON TOTAL SAMPLE OF 352 QUESTIONNAIRES FILLED OUT IN 1938 AND 349 IN 1952

		1938 Percentage of Respondents	1952 Percentage of Respondents
Question I	1	1.4	2.6
	2	7.4	14.0
	3	36.4	39.0
	4	14.5	10.3
	5	40.3	34.1
		100.0	100.0
Question II*	1	14.7	12.7
	2	23.8	25.8
	3	21.0	18.6
	4	4.1	4.3
	5	17.4	16.9
	6 (other)	19.0	21.7
		100.0	100.0
Question III			
(a)	Yes	89.2	88.8
	No	9.7	9.2
	Blank	1.1	2.0
		100.0	100.0
(b)	Yes	76.1	77.9
	Partly	1.7	2.9
	No	7.4	10.9
	Blank	14.8	8.3
		100.0	100.0
Question IV			
(a)	Yes	32.4	30.7
	Probably	2.0	1.7
	Partially	4.8	4.3
	No	13.4	17.8
	Probably not	.6	3.7
	Blank and doubtful	34.6	14.0
	Have not read it	12.2	27.8
		100.0	100.0
Filled-in questionnaires returned		58.4	67.8
Other replies		1.5	2.1
No reply		40.1	30.1
		100.0	100.0

*The base in this instance is total replies, not total respondents, since it is a multiple answer question. There were 689 replies in 1938; 655 in 1952.

Question II

This question dealt with the source of information on which the opinion expressed in Question I was based. By and large, the difference here between the two surveys is slight. For this and the following questions, only the general trends apparent from a comparison of the two surveys will be mentioned. Anyone interested in making a closer study on any points may consult the previous report for the detailed results of the previous questionnaire.

Among the "other" sources of opinion cited, "Discussion with colleagues" has risen considerably, whereas "Professional meetings" has fallen. "Personal experimentation" has dropped; "Personal experience," in contrast, has decidedly risen. The opinion expressed on Question I by those citing "Personal experience" as the source of their opinions is divided in about the same way as that of those who do not mention "Personal experience." Most respondents checked two or more sources of information on Question II.

Question III

Again the two surveys present practically parallel results. The overwhelming majority of psychologists feel that the investigation of ESP is legitimate, and most of these feel that it does fall within the province of academic psychology. Perhaps the chief innovation in the answers to the question, "Where else does the study of ESP belong?" is the volunteering for the first time of a new category, "Clinical Psychology," which polled more suggestions than any other category in its group.

Question IV

The number replying that they felt the present ESP work is in the proper direction remains stable. At the expense of a substantial drop in the number who were doubtful, there is a rise in the number who say they cannot answer because they have not kept up with the work and also a rise in the number who reply that the present ESP work is not, or is probably not, in the proper direction. The comments under Part *b*, the request for criticisms of methods, fall to a large degree into two categories. The leading category includes a variety of criticisms of the experimental design and a few suggestions for improving it. A somewhat smaller but substantial number of

respondents called attention to the possibility of bias on the part of investigators. In the first study, the possibility of fraud was raised by some people, but this is not true of the present study. The comments suggest the thought that the results are due to unconscious bias on the part of investigators convinced in advance of the reality and, to a degree, of the nature of the phenomenon under investigation. Other comments note the potential motivating value, perhaps on the unconscious level, of the knowledge that a substantial portion of the careers of some of the workers involved would seem to have been invested to small advantage should there turn out to be no such thing as ESP.

A thread which ran through a number of the comments, but which could hardly be represented in the tabulation without classifying a comment under more than one category, is this: The concept of ESP should be integrated with other known and far better established psychological concepts which may be related. There was also the idea that the experimental approach to the subject might better stem directly from known psychophysics, the study of minimal cues and thresholds.

A further thread which could not well be brought out in a separate category pertains to the schism between ESP and normal perception which seems to be demanded by most students of ESP (1). The term extrasensory perception was labeled by a few as being self-contradictory, a view expressed by a few respondents in the earlier survey.

Tabulated reporting of the returned questionnaires does not entirely give the flavor of the replies, nor is it entirely fair to the respondents who took the trouble to make thoughtful comments not to publish a representative sample of them. The following replies have been selected with this purpose in mind.

Comments Relative to the Term ESP and Its Implications, and the Relation of ESP to Normal Sense Perception

> Merely an unknown: I must make a comment. I really feel that it should be investigated as a psychological phenomenon and not as ESP. As soon as we label it as ESP we are making the implicit assumption that ESP exists.

* * *

Merely an unknown: Incidentally investigation of ESP perhaps *can never* be scientific because the term ESP alone implies *observation of the unobservable.*

* * *

An impossibility: The phenomena may be legitimate, but calling the phenomena investigated "ESP" decides in advance that they are of a certain (undefinable) nature. (A legitimate scientific undertaking) No, *not as "ESP."* An unbiased examination of the conditions under which certain phenomena of behavior occur, without acceptance of a mystical, semantically meaningless interpretation, *is* legitimate, although of very questionable long term usefulness and significance.

* * *

A remote possibility: The idea of reception without *receptors* of some kind I would have to reject. I would assume that there may be other kinds than those presently known.

* * *

Merely an unknown: The limits of sensory perception are not known well enough to enable us to say what is "extra."

* * *

An impossibility, as perception is sensory. Unless the awareness of *sub*conscious states can be called *perception.*

* * *

An impossibility: Could millions upon millions of persons have played bridge for years under rules and conditions that approach the constancy of an experiment? Motivation is high and rewards are often great. If there were such a phenomenon and it had any significance, it would have appeared and been discovered and described. Much of psychology is an elaboration of what has appeared under "natural" conditions. In the field of extrasensory perception nothing seems to appear which can be elaborated despite the expenditure of *much* money, time and energy. The results so far so meager even at their best, that serious question of their value at all can be raised.

* * *

An established fact: If we refer to the perception of sub-liminal cues, I believe it is an established fact.

* * *

A likely possibility: Every possibility of explaining findings in the light of existing theories and hypotheses of personality should be explored. Parsimony is extremely important in discussing findings of this nature.

* * *

An impossibility: If a scientist finds reason to believe that phenomena exist that are inconsistent with the basic postulates of known science it is his duty (a) to try to discover their explanation and, if necessary, show how theory must be extended to include these events. He must, however, avoid explanations outside the realm of nature, or the introduction of new constructs until the capacity for accepted constructs to explain the facts has been satisfactorily and exhaustively explored.

Comments Regarding Research Personnel in the Field of ESP

A reorientation toward the problem is urged; namely to place in the focus of inquiry the extra-sensory perceptionist. What are the quirks of personality that lead an otherwise sensible psychologist to continue to waste his time on so unrewarding a field of inquiry?

* * *

So many positive claims have turned out when carefully investigated to be based on artifacts that one is forced to the conclusion that there is something about this problem that leads the people who are attracted to it to come to false positive conclusions.

I suspect strongly that most of it is subject to the small but consistent bias in scoring errors (accounting for the small but consistent positive results) demonstrated in the paper that Kaufman and Sheffield read at the 1952 E.P.A.

* * *

(In the proper direction? Yes.) But we need more research by people who are not already committed to a "position."

* * *

It is the responsibility of Rhine and his "cult" to follow the pattern established by Kant.

* * *

Investigations should be done by researchers who are not inspired "cultists." Too much emphasis on trying to prove or defend the hypothesis. Whole movement takes on "cultist" proportions.

* * *

Still primarily a "one laboratory" phenomenon, raising a doubt as to adequacy of controls.

* * *

Too narrow and restricted, too naïve, in following "unconscious" wishes and the dramatic type of method and concept.

* * *

One restriction; it seems to be done by those who have wish fulfillment as their motive; and they subsequently obtain positive

results. I don't think impartial, skeptical people would waste their time on such research.

* * *

We found tallying by untrained persons showed systematic errors.

* * *

I feel that most of these investigations are designed to "prove" something the antithesis of science.

So improbable that the principal relevant topic for research is explanation of the present extent of interest in the question.

To my mind the frivolous failure on the part of research workers in the field of parapsychology to take all evidential factors fully into account in explaining the observed data with which they deal, is one important case in point.

* * *

A likely possibility: Like to see work scattered more in different laboratories . . .

* * *

A likely possibility: The seriousness, honesty, and reliability of people like Gardner Murphy, Gaither Pratt, C. R. Carpenter, etc. I think there's something in it because people I trust think so.

Comments Regarding the Legitimacy of ESP as a Field of Investigation

Any subject matter is legitimate and respectable for scientific inquiry. Science is a *method,* not a listing of proper and improper things to be studied. I don't believe in astrology, handwriting, etc., but I fight for their right to be the subject of Ph.D. theses. I applaud your sending out this questionnaire. Academic Psychology needs to re-define itself and clarify (modernize) its objectives.

* * *

(Legitimate scientific undertaking) in sense that one can study anything. No, in the sense that the study is likely to be barren and futile.

* * *

It will become part of academic psychology if and when we have an adequate theory to explain the phenomena.

Comments on Methodology

I would like to see research oriented around such phenomena as minimal cues, etc.

* * *

The actual ESP mechanism is never introduced experimentally into the test situation—nor is it likely to be. Consequently the experiments prove merely that chance alone cannot explain the number of "hits" made. The assumption, therefore, that experi-

ments demonstrate ESP is gratuitous. . . . Most absurd to me seems the so-called PK effect. How can ESP be accepted if wedded to PK?

* * *

Testable hypotheses cannot be formulated.

* * *

So much ground remains in the unplowed state in the area of sensory perception. Further advances here may eventually enlighten us on ESP.

* * *

Intensive work on "gifted" individuals should at the present stage be more useful than continued piling up of minute but "significant" deviations from chance.

* * *

Please take it from a psychologist in Reno, Nevada, that it hasn't worked as yet on roulette and I'm beginning to doubt if it will.

* * *

Suggest instrumental control of material perceived and of reaction of perceiver on a par with, say, atomic physics. (If ESP exists, it is at least equally important.)

* * *

Too much reliance on statistical considerations in card-sorting experiments and card guessing. More statistics needed on relation between possible ESP and normal and abnormal psychological states.

* * *

People vary a good deal as to sensitivity to ESP; effort should be made to discover what makes one person more sensitive than the other.

* * *

Experiments not too well designed; ESP could conceivably be a function of significant human relationships—at least it should not be assumed without proof that it is an automatic reaction to an external stimulus.

* * *

Belongs in the province of independent research without academic or other institutional restrictions.

* * *

Need for more established fact in so-called subliminal perception both in isolated and combined sensory areas.

* * *

My guess is that if so-called ESP is ever to be demonstrated it will be on the basis of a "tuning-in" similar to radio and there-

fore would involve preferably previous communicability between the two parties.

* * *

I don't read the JOURNAL OF PARAPSYCHOLOGY. What is needed is not more and more experimentation but some effort at fitting present ESP knowledge into theoretical frame.

* * *

Suggest study of psychic phenomena as reported by various persons who claim to have experienced "visions" and the like. Investigation of "haunted" places, under controlled conditions. Investigations of apparently unaccountable phenomena, to be explained on no other basis.

One respondent checked his replies twice, using different colored pencils, the one to indicate his present position and the other to indicate, "What my answers would have been had I completed this questionnaire fifteen years ago—five years after receiving my Ph.D." We scored his present position. It is interesting to note that he switched from an original opinion that ESP is an unknown to a present opinion that it is a likely possibility. He also switched from a "no" to a "yes" on the question of whether investigation of ESP is a "legitimate scientific undertaking."

Among the informal impressions reached by the writer as a result of reading the returned questionnaires are the following:

1. Interest in the subject is still concentrated in a relatively small group of psychologists, but their number has increased somewhat.

2. Awareness of current work at Duke University in any detail is strictly limited.

3. Many psychologists seem to hold the opinion that if there is any work being done except at Duke, the results are generally negative.

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BOOK REVIEWS

UNSTERBLICHKEIT, IHRE METAPHYSISCHE UND ANTROPOLOGISCHE BEDEUTUNG (Immortality, Its Metaphysical and Anthropological Meaning). By Aloys Wenzl. Munich: Leo Lehnen Verlag, 1951.

Aloys Wenzl is a well-known German philosopher, head of the Philosophy Department of the University of Munich. His book is mainly a discussion of the survival problem from the philosophical point of view. He discusses the difficulties of the problem, the possible content of it, and its relation to the problems of neighboring fields, such as biology and psychology. He considers its meaning for ethics, the philosophy of history, etc. There is a chapter called "Parapsychology," and in other parts of the book the author analyzes the problem of time. Both of the two latter topics are of interest to the students of our field.

Prof. Wenzl, like A. Messer, H. Driesch, and other distinguished German philosophers, has taken a positive attitude toward the question of the occurrence of psi phenomena. He states that ESP, especially telepathy, is a well-established fact. PK and haunting cases seem to him to be somewhat less so. He is skeptical but not completely negative toward precognition and materializations. He believes there is some evidence, though it is not final, for spirit communication.

It is a curious fact that Wenzl bases most of his conclusions on the work of German authors and ignores or misunderstands American and British parapsychology of the last two decades. Perhaps the German workers have the same feeling of curiosity about the widespread tendency in this country to take the last two decades of American and British publication for the whole of parapsychology.

Prof. Wenzl discusses very briefly some theories of psi. He agrees that ESP is not to be explained by physical agencies. He gives two alternative explanations for PK:

1. The cause could be the shifting of the fields of gravitation by some physical force; as in those reported cases when loss of weight by a medium indicates the consumption of physical energy.

2. "The motions of the elementary particles of a physical object are based on the laws of probability." (Wenzl has in mind Heisenberg's uncertainty principle and related theories.) "One may suppose that the normal biological and psychophysical actions are brought about by influencing the 'fields of probability.' If we could by means of 'suggestion' also influence a sufficient number of extrasomatic elements to take an a-normal, directed, uniform motion, the object as a whole would move, for example, upwards—against all probability." (Pp. 193)

If there are such very hypothetical things as materializations, Prof. Wenzl thinks, they can be interpreted by drawing an analogy with microphysical processes. The findings of modern physics itself include a kind of materialization; that is, there can be a transfer of waves into elementary particles like photons, electrons, etc. Perhaps mediums can send out "material waves" and they can be organized by some extrasomatic principles like the entelechy of the neovitalists.

This book has a very interesting analysis of the problem of dimensions, especially that of time. There have been many efforts to introduce a new hypothesis by starting either from the point of view of physics or that of parapsychology, but there is very much less effort to integrate facts and problems from all sources—physics, biology, normal and paranormal psychology—as Prof. Wenzl tries to do. He has the great advantage of feeling as much at home in physics as in psychology (he has studied both these and philosophy), and he makes good use of this fact.

We can give only a few lines of his very complex theory. He disagrees with the common tendency of physicists and parapsychologists to view all reality in static dimensions only. He recognizes the explicit dynamic trend in many findings of psychology, biology, and even physics itself which one cannot simply speculate away. He makes an effort to reconcile the old discrepancy between Eleatic and Heracleitic views, but in terms of modern scientific concepts. The Einstein-Minowski theory of the four-dimensional continuum, where time is a static dimension, is taken as a base, but Wenzl postulates a second time-dimension as a real order of succession, which is consistent with this static frame. He traces the emergence of this di-

mension in the findings of biology and psychology; for example, the potential development of a unique life cycle. It appears in depth psychology which shows that the Unconscious seems to be able to "see" simultaneously the wholes of very complex problems, even the themes of life. The Conscious sees them in its empirical time only in small successive parts. The human acts of free will require a real order of succession too.

Prof. Wenzl tries to prove that the concept of life after bodily death cannot be incorporated into the static four-dimensional system. His fifth dimension should make immortality a logically possible hypothesis in the modern picture of the world. After death the Conscious enters into the postulated dimension where the Unconscious partly participated before with its simultaneous awareness of complex contents.

The book has other chapters interesting for the problem of survival, but not for general parapsychology.

KARLIS OSIS

Dr. Osis, who has an article in this number of the JOURNAL, *is a member of the Duke Parapsychology Laboratory.*

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (—1), (—2), (—3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{}$no. of runs.) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

in this field report be true, the castle of our thought may need rebuilding from its foundations. Simple people talk glibly of telepathy, for example; yet if extra-sensory perception alone were established the whole scheme of modern thought crumbles into ruin. It would be nothing short of a scientific revolution. Science and philosophy would be under the necessity, for them a sad necessity, to seek new concepts for the interpretation of reality, to redraw their antiquated map of the human mind, and

> Cast their kingdoms old
> Into another mould.

—W. Macneile Dixon, *The Human Situation*

(By permission of Edward Arnold & Co.)

"THE MOST IMPORTANT AMONG ALL THE FACTS . . ."

. . . if, as I have also done, you have looked with some attention into the field of human faculty, into its still unexplored resources, into the testimony for the marvels that the submerged portion of our being reveals, you will not, I think, return with less amazement, but if possible in a state of still greater stupefaction. . . .

For the nature of mind, our own nature, the nature of every thing, of all reality, is here in the balance. We are deceived, indeed, if we fancy that our five senses exhaust the universe, or our present standpoint its many landscapes. In the soul's unvisited and sleeping parts it holds both faculties and powers not mentioned in the books of the historians, the manuals of the mathematicians or the physiologists. "The sensitive soul," as Hegel wrote, "oversteps the conditions of time and space; it beholds things remote, things long past and things to come." . . .

The study to which I refer has nothing to do with spiritualism or with religion. It is simply an enquiry into such occurrences as should not on our present theory of knowledge take place at all. And as Schopenhauer insisted "the phenomena under consideration are incomparably the most important among all the facts presented to us by the whole of experience from a philosophical point of view; so it is the duty of every man of science to get acquainted with them, and to study them thoroughly." How many of them, and how many of our divines are, in respect of this study, like the professor of Padua, who refused to look through Galileo's telescope, lest he might see what he did not wish to see, who dreaded its revelations. But what we prefer, like or dislike, alters nothing in nature. Palatable or unpalatable we must accept whatever lies in the path of our destiny. And if a tenth, a hundredth part of what competent observers

(Continued on inside back cover)